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PHILOSOPHICAL TRANSACTIONS.

XIX. *Experiments and observations on the developement of magnetical properties in steel and iron by percussion.* By WILLIAM SCORESBY, *Jun. Esq. Communicated by Sir* HUMPHRY DAVY, *Bart. P. R. S.*

Read March 7th, 1822.

DR. GILBERT, so early as the year 1600, discovered that iron became sensibly magnetic on being hammered and drawn out while lying in a north and south direction; but I am not aware that either Dr. GILBERT, or any other person, ever gave to iron or steel, by this process, any considerable lifting power. I cannot indeed discover that any magnetical effect, by hammering, has been produced, beyond that of occasioning a deviation in the compass needle, or of giving to floating bars, or needles, the power of conforming their position to that of the magnetic meridian.

In the course of some experiments made in the autumn of 1820, I succeeded in determining, in a great measure, the principal laws by which the developement and destruction of magnetism in iron, by percussion, scowering, filing, bending, &c. are governed. As the result of this investigation is al-

ready before the public,* the communication that I have now the honour of laying before the Royal Society, will be confined to the application of these laws to practical magnetism; and particularly to the construction of magnets, without the use of any magnetised substance.

In examining the magnetical effect of percussion on different kinds of iron and steel, two tests were employed; the weight of iron that the body would lift, and the quantity of deviation that it would produce on a magnetic needle when presented to it in a certain position, and at a certain distance. For the first test, common iron nails of different sizes were made use of: they were of the weights of 2, 4, $6\frac{1}{2}$, 14, 24, 37, 45, 88, 130, and 188 grains. For the purpose of securing a good and uniform contact with the magnetised bar, the oxide on the ends of the nails was removed by means of a fine file, and the extremities were then polished by rubbing them on a turkey stone. The second test I employed, consisted of a board two feet in length, with a longitudinal line down the middle divided into inches, and a sensible pocket compass. To guard against the effects of the magnetism of position, the central line of the board was placed exactly in an east and west direction by the compass, and as the board was laid horizontally on a table, this line was known to be in the plane of the magnetic equator, and consequently in a situation in which small bars of iron are not affected by the magnetism of position. In applying this simple apparatus as a measure of magnetism, the bar, whose magnetism was to be examined or compared, was laid along the central line of the board, with its north pole always nearest the compass.

* Edin. Phil. Trans, 1821.

The compass was placed with its centre at the commencement of the scale, so that its needle was exactly at right angles to the direction of the bar; and before the deviation took place, both its poles were equidistant from the bar. The distance was estimated by the scale on the board, and always represented the space between the north end, or nearest end of the bar, and the centre of the compass.

Three hammers were also employed: No. I. of 22 ounces; No. II. of 12 ounces; and No. III. of $2\frac{1}{2}$ ounces weight.

It may be remarked, that no exact value can be given to the number of blows, because, having struck them by the hand, they must have been liable to considerable variation in intensity; and also, that the lifting power cannot be considered as an exact measure of the magnetism developed by the process. The deviation, however, produced on the compass by the bodies presented to it at equal distances and in the same direction, was decisive as a comparative measure of their magnetisms; for, though the actual magnetic force is not given by this means, yet it is clear that a greater or less deviation, must be the effect of a greater or less magnetic force; while in bars of a similar kind and dimensions, an equal deviation must be the effect of equal magnetic forces.

I. *Experiments with a cylindrical bar of soft steel, for determining the effect of percussion, when the bar was held in a vertical position and resting upon a piece of metal not ferruginous. Length of the bar 6½ inches; diameter a quarter of an inch; weight 592 grains.*

Number of Blows.		Weight lifted.	Distance of Compass and Bar.	Deviation.	Hammer.
at each trial.	Total.				
1	1	2	3	8	II.
1	2	—	. . . .	10	. . . .
5	7	½	. . . .	12	. . . .
10	17	6½	. . . .	12½	. . . .
5	22	6½	. . . .	12½	I.

This bar was next hammered upon a large mass of free-stone: after twenty blows with the large hammer had been struck upon it, the deviation it was found had not increased, but still remained at 12½°.

Dr. GILBERT tried this experiment on iron; but instead of hammering it in the direction of the dipping needle, or in a vertical direction, which produces almost an equal effect, he placed it horizontally in the magnetic meridian, which, in London, is a plane elevated only 19½ degrees above the magnetic equator. Now, as my former experiments proved that hammering iron in the plane of the magnetic equator destroys its polarity, it is evident, that a very small part only of the full influence of percussion must have been obtained by Dr. GILBERT.

As magnetism in steel is more readily developed by the contact of magnetisable substances, and particularly if these substances be already magnetic, it occurred to me, that the magnetising effects of percussion might be greatly increased

by hammering the steel bar, with its lower end resting on the upper end of a large rod of iron or soft steel, both the masses being held in a vertical position: and that if the rod were first rendered magnetic by hammering, the effect on the bar would probably be augmented. The following experiments prove that these opinions were not incorrect.

II. *Experiments for determining the effect of percussion on a soft steel bar, when the bar was held in a vertical position and resting upon a parlour poker.*

[*a. Both the bar and the poker were first deprived of magnetism.*]

Number of Blows.		Weight lifted.	Distance of Compass and Bar.	Deviation.	No. of the Hammer.
at each trial.	Total.				
		Grains.	Inches.	°	
1	1	$6\frac{1}{2}$	3	18	II.
1	2	14	. . .	16	. . .
1	3	. . .	. . .	18	. . .
4	7	37	. . .	21	. . .
5	12	45	. . .	25	. . .
10	22	88	. . .	27	. . .
20	42	88	. . .	30	. . .
30	72	. . .	. . .	31	. . .
10	82	. . .	. . .	$31\frac{1}{2}$	. . .

The effect seemed now to be at a maximum, for more blows with the same hammer produced no alteration; but on substituting a larger hammer, the deviation was augmented.

[*b. Change of Hammer.*]

3	85	. . .	. . .	33	I.
5	90	130	. . .	34	
3	93	. . .	. . .	30!	

The bar was now inverted, so that the north pole was upward.

[*c. Change of the ends of the Bar.*]

1	. . .	0	3	5	II.
1	2	0	. . .	2	Poles changed.

In this stage of the experiments the poker had become strongly magnetic, and its magnetism was farther increased by hammering it upon another bar of iron.

[*d. The bar deprived of magnetism; but the poker rendered magnetic by hammering.*]

Number of Blows.		Weight lifted.	Distance of Compass and Bar.	Deviation.	Hammer.
at each trial.	Total.				
			Inches.	°	
1	1	. . .	8	16	I.
[The experiment repeated.]					
1	1	. . .	8	18	I.
1	1	. . .	8	17	II.
1	1	. . .	. . .	12½	III.
100	101	. . .	. . .	27	. . .
100	201	. . .	. . .	28½	. . .
2	203	. . .	. . .	28½	I.

The bruised end of the bar was now hammered horizontally until it formed a kind of cup; this being easily compressed by the hammer, the effect was increased.

[*e. Bar deprived of magnetism; poker hammered.*]

1	1	. . .	8	23	I.
1	1	. . .	8	22	II.
5	6	. . .	. . .	30	. . .
4	10	188	. . .	36?	I.
5	15	188	. . .	33	. . .

III. *For determining the effect of percussion on magnets properly tempered, a flat bar magnet of a blue temper was employed; it was* $7\frac{7}{10}$ *inches long,* $\frac{1}{3}$ *an inch broad, and* $\frac{1}{7}$*th thick, and weighed* 1170 *grains.*

[*a. The magnet held vertically on a rod of iron; South end upward.*

No. of Blows. at each trial.	No. of Blows. Total.	End of the Magnet upward.	Distance of the Compass.	Deviation.
			Inches.	°
0	0	. . .	8	45
5	5	S.	. . .	37
5	10	. . .	. . .	32
10	20	. . .	. . .	30
10	30	. . .	. . .	28
10	40	. . .	. . .	27
20	60	. . .	. . .	26
20	80	. . .	. . .	25
20	100	. . .	. . .	25
		[*b.* North end up.]		
0	0	. . .	8	25
1	1	N.	. . .	20
10	11	. . .	. . .	16
20	30	. . .	. . .	14
		[*c. Bar again magnetised and hammered upon a piece of pewter.*]		
0	0	. . .	8	50
10	10	S.	. . .	37
10	20	. . .	. . .	33
10	30	N.	. . .	27
10	40	. . .	. . .	26
20	60	. . .	. . .	24
20	80	. . .	. . .	24
20	100	S.	. . .	24
20	120	N.	. . .	28
20	140	. . .	. . .	28
20	160	S.	. . .	$28\frac{1}{2}$

The general results of these experiments are as follow:

1. A cylindrical bar of *soft steel*, $6\frac{1}{2}$ inches long, and weighing 592 grains, lifted, after repeated hammering on pewter and stone, $6\frac{1}{4}$ grains; but could not be made to lift a nail of 11 grains.

2. The same bar hammered vertically upon a parlour poker, the poker also held erect, after 22 blows, lifted, with the lower end, which was a North pole, 88 grains (2 *a*); and on using a larger hammer, received a considerable increase of power, producing a deviation of the compass, three inches distant, of $34\frac{1}{2}$ degrees (2 *b*): farther hammering, it was found, rather diminished than increased the effect (2 *b*). On the bar being inverted, so that the north pole was upward, the magnetism was very nearly destroyed by a single blow; while two blows changed the poles (2 *c*). Hammering the end of the bar in the plane of the magnetic equator also destroyed the polarity; but the effect was not fully produced until many blows had been struck.

When the poker had been previously hammered in a vertical position, an increase of magnetic effect on the bar was obtained; a single blow being now sufficient to enable the bar to lift about 20 grains: and when the end was hammered into a kind of cup, so as to be easily bruised, the bar was, by one blow, rendered capable of lifting between 30 and 40 grains (2 *e*). After 10 blows, the highest effect obtained in all the experiments was produced, the same bar readily lifting a nail of 188 grains, being nearly one-third of its own weight! (2 *e*).

The magnetism by percussion was found, by subsequent experiments, to be augmented when the length of the bars

was increased; thus a quarter-inch cylindrical bar of steel five inches long, after receiving 20 smart blows, produced a deflection of the needle, at the distance of three inches, of 13°, and lifted 6½ grains. Another piece of the same bar 7¾ inches long, after similar treatment, produced a deviation of 24°, and lifted 45 grains; and a third bar of the same kind 12 inches long, after twenty similar blows, occasioned a deviation of the compass of 33°, and easily lifted 88 grains. The shortest bar, it was observed, received the full effect by the two first blows; but the others continued to increase in energy as the percussion was continued. These bars did not receive a power equal to that first used; the cause was probably their greater hardness.

3. A *strong magnet* properly tempered, was injured in whatever position it was hammered; but most rapidly when the north pole was upward. After no farther diminution of its magnetism could be produced with the south end upward, a quick loss of power was effected by hammering it with the north pole upward (3 *b*). But after the magnetism had been reduced to a certain extent, by hammering in both positions, the power became nearly stationary; so that on striking it in any position with the same hammer, very little change of intensity occurred (3 *c*.).

Besides these results, I may mention the effect of percussion on soft steel magnets; on soft iron not magnetised; and on cast iron.

4. A *soft steel magnet* capable of lifting upwards of 1000 grains, when placed vertically upon the poker with its north pole upward, had its magnetism destroyed by five blows.

5. A bar of *soft iron*, of the same size and form as the

steel bar used in the first and second series of experiments, and weighing about 600 grains, was hammered for a considerable time while held vertically upon the poker. The greatest effect which I could produce with the large hammer, was a deflection of the compass needle, at the distance of three inches, of thirteen degrees; in this state it lifted a nail of 6½ grains, but refused one of 11 grains weight.

6. A *cast iron* bar of the same size and form as the last, became capable of lifting a nail of 37 grains weight. After it had acquired this power, its magnetism was nearly destroyed by five blows with the north pole upward.

The strong magnetising effect of percussion on soft steel, induced me to apply the property to the formation of magnets. For this purpose I procured two bars of soft steel, 30 inches long and an inch broad; also six other flat bars of soft steel 8 inches long and half an inch broad, and a large bar of soft iron. The large steel and iron bars were not however absolutely necessary, as common pokers answer the purpose very well; but I was desirous to accelerate the process by the use of substances capable of aiding the developement of the magnetical properties in steel. The large iron bar was first hammered in a vertical position. It was then laid on the ground with its acquired south pole towards the south, and upon this end of it the large steel bars were rested while they were hammered; they were also hammered upon each other. On the summit of one of the large steel bars, each of the small bars, held also vertically, was

hammered in succession, and in a few minutes they had all acquired considerable lifting powers. Two of the smaller bars, connected by two short pieces of soft iron in the form of a parallelogram, were now rubbed with the other four bars in the manner of CANTON. These were then changed for two others: and these again for the last two. After treating each pair of bars in this way for a number of times, and changing them whenever the manipulations had been continued for about a minute, the whole of the bars were at length found to be magnetised to saturation; each pair readily lifting above eight ounces!

In accomplishing this object, I took particular care that no magnetic substance was used in the process. All the bars were freed of magnetism before the experiment, so that none of them, not even the largest, produced a deviation of five degrees on the compass at three inches distance. The hammers were also carefully examined. Any bars which had been strongly magnetised, and had had their magnetisms destroyed or neutralised (either by hammering, heating, or by the simultaneous contact of the two poles of another magnet placed transversely,) I always found had a much greater facility for receiving polarity in the same direction as before, than the contrary. Hence it generally happened, that one blow with the original north end downward, produced as much effect as two or three blows did with the original south end downward. I also observed that the polarity of *pokers*, generally supposed to be permanent and considerable in intensity, was rather transient and weak: for in no instance did I meet with a poker, the magnetism of which I could not

destroy by a blow or two with a hammer, on the point; and, in general, two blows, even when the poker was held in the hand, and not rested upon any thing, were sufficient to invert the poles.

Liverpool,
12th February, 1822.

XX. *On the Alloys of steel. By* J. Stodart, *Esq. F. R. S. and Mr.* M. Faraday, *Chemical Assistant in the Royal Institution. Communicated by* J. Stodart, *Esq. F. R. S.*

Read March 21, 1822.

The alloys of steel made on a small scale in the laboratory of the Royal Institution proving to be good, and the experiments having excited a very considerable degree of interest both at home and abroad, gave encouragement to attempt the work on a more extended scale, and we have now the pleasure of stating, that alloys similar to those made in the Royal Institution, have been made for the purpose of manufacture; and that they prove to be, in point of excellence, in every respect equal, if not superior, to the smaller productions of the laboratory. Previous however, to extending the work, the former experiments were carefully repeated, and to the results were added some new combinations, namely, steel with palladium, steel with iridium, and osmium, and latterly, steel with chromium. In this last series of experiments we were particularly fortunate, having by practice acquired considerable address in the management of the furnaces, and succeeded in procuring the best fuel for the purpose. Notwithstanding the many advantages met with in the laboratory of the Royal Institution, the experiments were frequently rendered tedious from causes often unexpected, and sometimes difficult to overcome; among these, the failure of crucibles was perhaps the most perplexing. We have

never yet found a crucible capable of bearing the high degree of temperature required to produce the perfect reduction of titanium; indeed we are rather disposed to question whether this metal has ever been so reduced: our furnaces are equal* (if any are) to produce this effect, but hitherto we have failed in procuring a crucible.

The metals that form the most valuable alloys with steel are silver, platina, rhodium, iridium and osmium, and palladium; all of these have now been made in the large way, except indeed the last named. Palladium has, for very obvious reasons, been used but sparingly; four pounds of steel with $\frac{1}{100}$ part of palladium, has however been fused at once, and the compound is truly valuable, more especially for making instruments that require perfect smoothness of edge.

We are happy to acknowledge the obligations due from us to Dr. WOLLASTON, whose assistance we experienced in every stage of our progress, and by whom we were furnished with all the scarce and valuable metals; and that with a liberality which enabled us to transfer our operations from the laboratory of the chemist, to the furnace of the maker of cast steel.

In making the alloys on a large scale, we were under the necessity of removing our operations from London to a steel furnace at Sheffield; and being prevented by other avocations from giving personal attendance, the superintendence of the work was consequently intrusted to an intelligent and confidential agent. To him the steel, together with the alloying metals in the exact proportion, and in the most favourable state for the purpose, was forwarded, with instructions to see

* We have succeeded in fusing in these furnaces rhodium, and also, though imperfectly, platinum in crucibles.

the whole of the metals, and nothing else, packed into the crucible, and placed in the furnace, to attend to it while there, and to suffer it to remain for some considerable time in a state of thin fusion, previous to its being poured out into the mould. The cast ingot was next, under the same superintendence, taken to the tilting mill, where it was forged into bars of a convenient size, at a temperature not higher than just to render the metal sufficiently malleable under the tilt hammer. When returned to us, it was subjected to examination both mechanical and chemical, as well as compared with the similar products of the laboratory From the external appearance, as well as from the texture of the part when broken by the blow of the hammer, we were able to form a tolerably correct judgment as to its general merits; the hardness, toughness, and other properties, were farther proved by severe trials, after being fashioned into some instrument, or tool, and properly hardened and tempered.

It would prove tedious to enter into a detail of experiments made in the Royal Institution; a brief notice of them will at present be sufficient. After making imitations of various specimens of meteoric iron by fusing together pure iron and nickel, in proportions of 8 to 10 per cent, we attempted making an alloy of steel with silver, but failed, owing to a superabundance of the latter metal; it was found, after very many trials, that only the $\frac{1}{500}$ part of silver would combine with steel, and when more was used a part of the silver was found in the form of metallic dew lining the top and sides of the crucible; the fused button itself was a mere mechanical mixture of the two metals, globules of silver being pressed out of the mass by contraction in cooling, and more of these globules being forced out by the hammer in forging; and far-

ther, when the forged piece was examined, by dissecting it with diluted sulphuric acid, threads or fibres of silver were seen mixed with the steel, having something of the appearance of steel and platina when united by welding: but when the proportion of silver was only $\frac{1}{500}$ part, neither dew, globules, nor fibres appeared, the metals being in a state of perfect chemical combination, and the silver could only be detected by a delicate chemical test.

With platina and rhodium, steel combines in every proportion; and this appears also to be the case with iridium and osmium: from 1 to 80 per cent. of platina was perfectly combined with steel, in buttons of from 500 to 2000 grains. With rhodium, from 1 to 50 per cent. was successfully used. Equal parts by weight of steel and rhodium gave a button, which, when polished, exhibited a surface of the most exquisite beauty: the colour of this specimen is the finest imaginable for a metallic mirror, nor does it tarnish by long exposure to the atmosphere: the specific gravity of this beautiful compound is 9.176. The same proportion of steel and platina gave a good button, but a surface highly crystalline renders it altogether unfit for a mirror. In the laboratory we ascertained that, with the exception of silver, the best proportion of the alloying metal, when the object in view was the improvement of edge tools, was about $\frac{1}{100}$ part, and in this proportion they have been used in the large way. It may be right to notice, that in fusing the metals in the laboratory no flux whatever was used, nor did the use of any ever appear to be required.

Silver being comparatively of little value with some of the alloying metals, we were disposed to make trial with it as the first experiment in the large way. 8lbs. of very good

Indian steel was sent to our agent, and with it $\frac{1}{100}$ part of pure silver: a part of this was lost owing to a defect in the mould; a sufficient quantity was however saved, to satisfy us as to the success of the experiment. This, when returned, had the most favourable appearance both as to surface and fracture; it was harder than the best cast steel, or even than the Indian wootz, with no disposition whatever to crack, either under the hammer, or in hardening. Some articles, for various uses, have been made from this alloy; they prove to be of a very superior quality; its application will probably be extended not only to the manufacture of cutlery, but also to various descriptions of tools; the trifling addition of price cannot operate against its very general introduction. The silver alloy may be advantageously used for almost every purpose for which good steel is required.

Our next experiment made in the large way, was with steel and platina. 10lbs. of the same steel, with $\frac{1}{100}$ part of platina, the latter in the state produced by heating the ammonia muriate in a crucible to redness, was forwarded to our agent, with instructions to treat this in the same way as the last named metals. The whole of this was returned in bars remarkable for smoothness of surface and beauty of fracture. Our own observation, as well as that of the workmen employed to make from it various articles of cutlery, was, that this alloy, though not so hard as the former, had considerably more toughness: this property will render it valuable for every purpose where tenacity, as well as hardness, is required; neither will the expense of platina exclude it from a pretty general application in the arts; its excellence will much more than repay the extra cost.

The alloys of steel with rhodium have also been made in the large way, and are perhaps the most valuable of all; but these, however desirable, can never, owing to the scarcity of the metal, be brought into very general use. The compound of steel, iridium and osmium, made in the large way, is also of great value; but the same cause, namely, the scarcity and difficulty of procuring the metals, will operate against its very general introduction. A sufficient quantity of these metals may, perhaps, be obtained to combine with steel for the purpose of making some delicate instruments, and also as an article of luxury, when manufactured into razors. In the mean time, we have been enabled, repeatedly, to make all these alloys (that with palladium excepted) in masses of from 8 to 20lbs. each; with such liberality were we furnished with the metals from the source already named.

A point of great importance in experiments of this kind was, to ascertain whether the products obtained were exactly such as we wished to produce. For this purpose, a part of each product was analysed, and in some cases the quantity ascertained; but it was not considered necessary in every case to verify the quantity by analysis, because, in all the experiments made in the laboratory, the button produced after fusion was weighed, and if it fell short of the weight of both metals put into the crucible, it was rejected as imperfect, and put aside. When the button gave the weight, and on analysis gave proofs of containing the metal put in to form the alloy, and also on being forged into a bar and acted on by acids, presented an uniform surface, we considered the evidence of its composition as sufficiently satisfactory. The processes of analysis, though simple, we shall briefly state; the informa-

tion may be desirable to others who may be engaged on similar experiments; and farther, may enable every one to detect any attempt at imposition. It would be very desirable at present, to possess a test as simple, by which we could distinguish the wootz, or steel of India, from that of Europe; but this, unfortunately, requires a much more difficult process of analysis.

To ascertain if platina is in combination with steel, a small portion of the metal, or some filings taken from the bar, is to be put into dilute sulphuric acid; there will be rapid action; the iron will be dissolved, and a black sediment left, which will contain carbon, hydrogen, iron and platina; the carbon and hydrogen are to be burnt off, the small portion of iron separated by muriatic acid, and the residuum dissolved in a drop or two of nitro-muriatic acid. If a piece of glass be moistened with this solution, and then heated by a spirit lamp and the blow pipe, the platina is reduced, and forms a metallic coating on the glass.

In analysing the alloy of steel and silver, it is to be acted on by dilute sulphuric acid, and the powder boiled in the acid; the silver will remain in such a minute state of division, that it will require some time to deposit. The powder is then to be boiled in a small portion of strong muriatic acid;* this will dissolve the iron and silver, and the latter will fall down as a chloride of silver on dilution with water; or the powder may be dissolved in pure nitric acid, and tested by muriatic acid and ammonia.

* Although it is a generally received opinion that muriatic acid does not act on silver, yet that is not the case; pure muriatic acid dissolves a small portion of silver very readily.

The alloy of steel and palladium, acted on by dilute sulphuric acid, and boiled in that acid, left a powder which, when the charcoal was burnt from it, and the iron partly separated by cold muriatic acid, gave on solution in hot muriatic acid, or in nitro-muriatic acid, a muriate of palladium; the solution, when precipitated by prussiate of mercury, gave prussiate of palladium; and a glass plate moistened with it and heated to redness, became coated with metallic palladium.

The residuum of the rhodium alloy obtained by boiling in diluted sulphuric acid, had the combustible matter burnt off, and the powder digested in hot muriatic acid: this removed the iron; and by long digestion in nitro-muriatic acid, a muriate of rhodium was formed, distinguishable by its colour, and by the triple salt it formed with muriate of soda.

To analyse the compound of steel with iridium and osmium, the alloy should be acted on by dilute sulphuric acid, and the residuum boiled in the acid; the powder left is to be collected and heated with caustic soda in a silver crucible to dull redness for a quarter of an hour, the whole to be mixed with water, and having had excess of sulphuric acid added, it is to be distilled, and that which passes over condensed in a flask: it will be a solution of oxide of osmium, will have the peculiar smell belonging to that substance, and will give a blue precipitate with tincture of galls. The portion in the retort being then poured out, the insoluble part is to be washed in repeated portions of water, and then being first slightly acted on by muriatic acid to remove the iron, is to be treated with nitro-muriatic acid, which will give a muriate of iridium.

In these analyses, an experienced eye will frequently

perceive, on the first action of the acid, the presence of the alloying metal. When this is platina, gold, or silver, a film of the metal is quickly formed on the surface of the acid.

Of alloys of platina, palladium, rhodium, and iridium and osmium, a ready test is offered when the point is not to ascertain what the metal is, but merely whether it be present or not. For this purpose we have only to compare the action of the same acid on the alloy and on a piece of steel; the increased action on the alloy immediately indicates the presence of the metal; and by the difference of action, which on experience is found to be produced with the different metals, a judgment may be formed even of the particular one present.

The order in which the different alloys stand with regard to this action, is as follows: steel, cromium alloy, silver alloy, gold alloy, nickel alloy, rhodium alloy, iridium and osmium alloy, palladium alloy, platina alloy. With similar acid the action on the pure steel was scarcely perceptible; the silver alloy gave very little gas, nor was the gold much acted on. All the others gave gas copiously, but the platina alloy in most abundance.

In connection with the analysis of these alloys, there are some very interesting facts to be observed during the action of acids on them, and perhaps none of these are more striking than those last referred to. When the alloys are immersed in diluted acid, the peculiar properties which some of them exhibit, not only mark and distinguish them from common steel, and from each other, but also give rise to some considerations on the state of particles of matter of different

kinds when in intimate mixture or in combination, which may lead to clearer and more perfect ideas on this subject.

If two pieces, one of steel, and one steel alloyed with platina, be immersed in weak sulphuric acid, the alloy will be immediately acted on with great rapidity and the evolution of much gas, and will shortly be dissolved, whilst the steel will be scarcely at all affected. In this case, it is hardly possible to compare the strength of the two actions. If the gas be collected from the alloy and from the steel for equal intervals of time, the first portions will surpass the second some hundreds of times.

A very small quantity of platina alloyed with steel confers this property on it: $\frac{1}{400}$ increased the action considerably; with $\frac{1}{200}$ and $\frac{1}{100}$ it was powerful; with 10 per cent. of platina it acted, but not with much power; with 50 per cent. the action was not more than with steel alone; and an alloy of 90 platina with 20 steel was not affected by the acid.

The action of other acids on these alloys is similar to that of sulphuric acid, and is such as would be anticipated: dilute muriatic acid, phosphoric acid, and even oxalic acid, acted on the platina alloy with the liberation of more gas than from zinc; and tartaric acid and acetic acid rapidly dissolved it. In this way chalybeate solutions, containing small portions of protoxide of iron, may be readily obtained.

The cause of the increased action of acids on this and similar alloys, is, as the President of this Society suggested to us, probably electrical. It may be considered as occasioned by the alloying metal existing in such a state in the mass, that its particles form voltaic combinations with the particles of

steel, either directly, or by producing a definite alloy, which is diffused through the rest of the steel; in which case the whole mass would be a series of such voltaic combinations: or it may be occasioned by the liberation, on the first action of the acid, of particles which, if not pure platina, contain, as has been shown, a very large proportion of that metal, and which, being in close contact with the rest of the mass, form voltaic combinations with it in a very active state: or, in the third place, it may result from the iron being mechanically divided by the platina, so that its particles are more readily attacked by the acid, analogous to the case of proto-sulphuret of iron.

Although we have not been able to prove by such experiments, as may be considered strictly decisive, to which of these causes the action is owing, or how much is due to any of them, yet we do not hesitate to consider the second as almost entirely, if not quite, the one that is active. The reasons which induce us to suppose this to be the true cause of the action, rather than any peculiar and previous arrangement of the particles of steel and platina, or than the state of division of the steel, are, that the two metals combine in every proportion we have tried, and do not, in any case, exhibit evidences of a separation between them, like those, for instance, which steel and silver exhibit; that when, instead of an acid, weaker agents are used, the alloy does not seem to act with them as if it was a series of infinitely minute voltaic combinations of steel and platina, but exactly as steel alone would do; that the mass does not render platina wire more negative than steel, as it probably in the third case would do; that it does not rust more rapidly in a damp atmosphere; and that when

placed in saline solutions, as muriate of soda, &c., there is no action takes place between them. In such cases it acts just like steel; and no agent that we have as yet tried, has produced voltaic action that was not first able to set a portion of the platina free by dissolving out the iron.

Other interesting phenomena exhibited by the action of acid on these steels, are the differences produced when they are hard and when soft. Mr. DANIEL, in his interesting paper on the mechanical structure of iron, published in the Journal of Science, has remarked, that pieces of hard and soft steel being placed in muriatic acid, the first required five fold the time of the latter to saturate the acid; and that when its surface was examined, it was covered with small cavities like worm-eaten wood, and was compact and not at all striated, and that the latter presented a fibrous and wavy texture.

The properties of the platina alloy have enabled us to observe other differences between hard and soft steel equally striking. When two portions of the platina alloy, one hard and one soft, are put into the same diluted sulphuric acid and suffered to remain for a few hours, then taken out and examined, the hard piece presents a covering of a metallic black carbonaceous powder, and the surface is generally slightly fibrous, but the soft piece, on examination, is found to be covered with a thick coat of grey metallic plumbaginous matter, soft to the touch, and which may be cut with a knife, and its quantity seven or eight times that of the powder on the hard piece: it does not appear as if it contained any free charcoal, but considerably resembles the plumbaginous powder Mr. DANIEL describes as obtained by the action of acid on cast iron.

The same difference is observed if pure steel be used, but it is not so striking; because, being much less rapidly attacked by the acid, it has to remain longer in it, and the powder produced is still farther acted on.

The powder procured from the soft steel or alloy in these experiments, when it has not remained long in the acid, exactly resembles finely divided plumbago, and appears to be a carburet of iron, and probably of the alloying metal also. It is not acted on by water, but in the air the iron oxidates and discolours the substance. When it remains long in the acid, or is boiled in it, it is reduced to the same state as the powder from the hard steel or alloy.

When any of these residua are boiled in diluted sulphuric or muriatic acid, protoxide of iron is dissolved, and a black powder remains unalterable by the farther action of the acid; it is apparently in greater quantity from the alloys than from pure steel, and when washed, dried, and heated to 300° or 400° in the air, burns like pyrophorus, with much fume; or if lighted, burns like bitumen, and with a bright flame; the residuum is protoxide of iron, and the alloying metal. Hence, during the action of the acid on the steel, a portion of hydrogen enters into combination with part of the metal and the charcoal, and forms an inflammable compound not acted upon by the acid.

Some striking effects are produced by the action of nitric acid on these powders. If that from pure steel be taken, it is entirely dissolved; and such is also the case if the powder be taken from an alloy, the metal of which is soluble in nitric acid; but if the powder is from an alloy, the metal of which is not soluble in nitric acid, then a black residuum is left not

touched by the acid; and which, when washed and carefully dried, is found, when heated, to be deflagrating; and with some of the metals, when carefully prepared, strongly explosive.

The fulminating preparation obtained from the platina alloy, when dissolved in nitro-muriatic acid, gave a solution containing much platina, and very little iron. When a little of it was wrapped in foil and heated, it exploded with much force, tearing open the foil, and evolving a faint light. When dropped on the surface of heated mercury, it exploded readily at 400° of FAHRENHEIT, but with difficulty at 370°. When its temperature was raised slowly, it did not explode, but was decomposed quietly. When detonated in the bottom of a hot glass tube, much water and fume were given off, and the residuum collected was metallic platina with a very little iron and charcoal. We are uncertain how far this preparation resembles the fulminating platina of Mr. EDMUND DAVY.

In these alloys of steel the differences of specific gravity are not great, and may probably be in part referred to the denser state of the metals from more or less hammering; at the same time it may be observed, that they are nearly in the order of the specific gravities of the respective alloying metals.

The alloys of steel with gold, tin, copper, and chromium, we have not attempted in the large way. In the laboratory, steel and gold were combined in various proportions; none of the results were so promising as the alloys already named, nor did either tin or copper, as far as we could judge, at all improve steel. With titanium we failed, owing to the imperfection of crucibles. In one instance, in which the fused

button gave a fine damask surface, we were disposed to attribute the appearance to the presence of titanium; but in this we were mistaken; the fact was, we had unintentionally made wootz. The button, by analysis, gave a little silex and alumine, but not an atom of titanium; menachanite, in a particular state of preparation, was used: this might possibly contain the earths or their basis, or they may have formed a part of the crucible.

M. BERTHIER, who first made the alloy of steel and chromium,* speaks very favourably of it. We have made only two experiments. 1600 grains of steel, with 16 of pure chrome, were packed into one of the best crucibles, and placed in an excellent blast furnace: the metals were fused, and kept in that state for some time. The fused button proved good and forged well: although hard, it showed no disposition to crack. The surface being brightened, and slightly acted on by dilute sulphuric acid, exhibited a crystalline appearance; the crystals, being elongated by forging, and the surface again polished gave, by dilute acid, a very beautiful damask. Again, 1600 grains of steel with 48 of pure chrome were fused: this gave a button considerably harder than the former. This too was as malleable as pure iron, and also gave a very fine damask. Here a phenomenon rather curious was observed: the damask was removed by polishing, and restored by heat without the use of any acid. The damasked surface, now coloured by oxidation, had a very novel appearance: the beauty was heightened by heating the metal in a way to exhibit all the colours caused by oxidation, from pale straw to blue, or from about 430 to 600° of FAHRENHEIT. The blade

* Annales de Chimie, XVII. 55.

of a sabre, or some such instrument, made from this alloy, and treated in this way, would assuredly be beautiful, whatever its other properties might be; for of the value of the chrome alloy for edge tools we are not prepared to speak, not having made trial of its cutting powers. The sabre blade, thus coloured, would amount to a proof of its being well tempered; the blue back would indicate the temper of a watch spring, while the straw colour towards the edge would announce the requisite degree of hardness. It is confessed, that the operation of tempering any blade of considerable length in this way, would be attended with some difficulty.

In the account now given of the different alloys, only one triple compound is noticed, namely, steel, iridium and osmium; but this part of the subject certainly merits farther investigation, offering a wide and interesting field of research. Some attempts to form other combinations of this description proved encouraging, but we were prevented, at the time, by various other avocations, from bestowing on them that attention and labour they seemed so well to deserve.*

It is a curious fact, that when pure iron is substituted for steel, the alloys so formed are much less subject to oxidation. 3 per cent. of iridium and osmium fused with some pure iron, gave a button, which when forged and polished was exposed, with many other pieces of iron, steel, and alloys, to a moist atmosphere: it was the last of all showing any rust. The colour of this compound was distinctly blue; it had the property of becoming harder when heated to redness and quenched in a cold fluid. On observing this steel-like character, we sus-

* It is our intention to continue these experiments at every opportunity, but they are laborious, and require much time and patience.

pected the presence of carbon; none however was found, although carefully looked for. It is not improbable that there may be other bodies, besides charcoal, capable of giving to iron the properties of steel; and though we cannot agree with M. BOUSSINGAULT,* when he would replace carbon in steel by silica or its base, we think his experiments very interesting on this point, which is worthy farther examination.

We are not informed as to what extent these alloys, or any of them, have been made at home, or to what uses they have been applied; their more general introduction in the manufacture of cutlery would assuredly add to the value, and consequently to the extension of that branch of trade. There are various other important uses to which the alloys of steel may advantageously be applied. If our information be correct, the alloy of silver, as well as that of platina, has been to some considerable extent in use at His Majesty's Mint. We do know, that several of the alloys have been diligently and successfully made on the Continent; very good specimens of some of them having been handed to us; and we are proud of these testimonies of the utility of our endeavours.

To succeed in making and extending the application of these new compounds, a considerable degree of faithful and diligent attention will be required on the part of the operators. The purity of the metals intended to form the compound is essential; the perfect and complete fusion of both must, in every case, be ascertained: it is farther requisite, that the metals be kept for some considerable time in the state of thin fusion; after casting, the forging is with equal care to be attended to; the metal must on no account be overheated;

* Annales de Chimie, XVI. 1.

and this is more particularly to be attended to when the alloying metal is fusible at a low temperature, as silver. The same care is to be observed in hardening: the article is to be brought to a cherry-red colour, and then instantly quenched in the cold fluid.

In tempering, which is best performed in a metallic bath properly constructed, the bath will require to be heated for the respective alloys, from about 70° to 100° of FAHRENHEIT above the point of temperature required for the best cast steel. We would farther recommend, that this act of tempering be performed twice; that is, at the usual time before grinding, and again just before the last polish is given to the blade. This second tempering may perhaps appear superfluous, but upon trial its utility will be readily admitted. We were led to adopt the practice by analogy, when considering the process of making and tempering watch springs.

XXI. *Some observations on the buffy coat of the Blood, &c.* By JOHN DAVY, *M. D. F. R. S.*

Read April 18, 1822.

1. THE buffy coat, as it is technically called, which appears on blood drawn from persons labouring under inflammatory disease, has been referred, by Mr. HEWSON, to two circumstances; to increased tenuity of the blood, and to its slow coagulation.* But, in the explanation which is now usually given of the phenomenon, it is attributed to the latter circumstance, to the entire neglect of the former. The reverse of this, I believe, would be more correct; for in cases in which the inflammatory diathesis is best marked, the separation of the red particles from the blood drawn is most rapid, often occurring in one or two minutes; and, in some diseases, particularly in erysipelas, the blood taken from a vein coagulates as rapidly as healthy blood, and yet exhibits the buffy coat. In instances of this kind, when I have watched the coagulation of the blood, the red particles have subsided in the short space of two minutes, leaving a supernatant stratum of coagulable lymph, transparent and liquid. The buffy coat, in these instances, did not appear on the blood collected in the common bleeding cups, only when small vessels, as wine-glasses, or small gallipots were used, and quickly filled, and instantly set aside to rest. May it not, therefore, be inferred generally,

* "An Experimental Inquiry into the Properties of the Blood, with Remarks on some of its Morbid Appearances." By WILLIAM HEWSON, F. R. S. London, 1771, p. 56 and 59.

that the buffy coat is principally owing, not to the slow coagulation of the blood on which it appears, but to its increased tenuity; or, in other words, to the diminished viscidity of coagulable lymph, the effect of morbid vascular action connected with the inflammatory diathesis?

Mr. HEY has asserted, in opposition to Mr. HEWSON, that the coagulable lymph is not itself attenuated in inflammatory diseases, and that when it appears to be so, it is from dilution with serum.* Were this opinion correct, such blood should be of low specific gravity, which it is not, as I have satisfied myself by numerous experiments, made both in this country and in Ceylon. In general, I have found the blood, on which a buffy coat has appeared, of higher specific gravity than healthy blood.

2. It is an opinion pretty generally prevalent, that the age of these morbid adhesions, which are so frequently met with in the dissection of bodies, connecting together serous membranes, may be guessed at by their strength: thus, weak adhesions are usually considered of recent origin, and firm adhesions, of long standing. Is this opinion correct? And, does it agree with the properties of coagulable lymph, of which these adhesions are principally formed? Many circumstances, of which it will be sufficient to mention a few, lead to a reply in the negative.

Wounds, it is well known, that heal by the *first intention*, are often firmly united in twenty-four hours.

I have observed strong adhesions formed in the same space of time between the surfaces of the pleura, in consequence of inflammation artificially excited. An instance may be given.

* Observations on the Blood, by WILLIAM HEY, F. R. S. London, 1779. p. 47-49.

At Colombo, in the island of Ceylon, in January, 1819, I made the following experiment on a young dog nearly full grown. An opening was made with a scalpel between the ribs of the right side of the chest, through which about a scruple of arrack was injected into the cavity of the pleura. The lung was slightly wounded; air passed freely through the opening, and a little frothy blood was discharged. The animal, at first, seemed to suffer much pain, and to be very languid; but, left to itself, it gradually recovered, and in the course of the day took some food. At the expiration of twenty-four hours it was hot, but apparently not suffering pain; it was running about, and the wound was closed. It was now killed, and almost immediately examined. A good deal of vascular coagulated lymph was found under the skin round the wound, connecting the cutis and the intercostal muscles; the adjoining cellular membrane was inflamed; some bloody serum was effused into the right cavity of the chest; many pretty firm and long adhesions had formed between the pleura pulmonalis and costalis, both which were of a reddish hue; there were many adhesions too between the pleura and pericardium; and the pericardium was inflamed, and generally adherent to the surface of the heart.

The coagulated lymph of the buffy coat of the blood may be used as an illustration and confirmation of the short time in which strong adhesions may form. Liquid, when the blood is drawn, coagulable lymph gradually becomes, first viscid, and afterwards solid. In the viscid state, as I have frequently observed, when it is still transparent, it has the tenacity of mucus, and admits of being drawn out into fibres and bands, which, soon becoming solid and opaque, very well represent

the ordinary adhesions of the lungs; and in a very few hours attain their maximum of strength.

This viscidity, which coagulable lymph acquires in passing from a liquid to a solid form, has not, that I am aware, been noticed by authors; and the formation of adhesions is usually explained without reference to this quality.*

Though I believe the common opinion to be untenable, that the age of adhesions can be decided by their strength, it is far from my intention to maintain that they do not become firmer in progress of time, or that their duration may not occasionally be conjectured from their appearance and resistance.

3. It is believed by many, that the small portions of serous fluid which are found after death in the cavities of serous membranes, especially in the pericardium and the ventricles of the brain, may have been poured out after the cessation of life.† I am not aware that this opinion is other than hypothetical, or that it is supported by any precise facts. As a theoretical conclusion its correctness seems doubtful. I have endeavoured to put it to the test of experiment, and the result has not been favourable to it. I have notes of three different experiments on dogs, which were made in Ceylon in 1818, all which seem to show that, under ordinary circumstances, no effusion of serum, or exudation so as to occasion accumulation, takes place after death. The experiments were briefly the following. In each instance, a healthy dog was

* Vide "The Morbid Anatomy of some of the most important Parts of the Human Body, by MATTHEW BAILLIE, M. D. F. R. S. &c." 5th edit. p. 6.

† SAUVAGES' Nosologia Method. *Ephialtes ex Hydrocephalo.*

Cours d'Anatomie Medicale, par ANTOINE PORTAL, tom. IV. p. 54. 8vo. Paris, 1803.

suddenly killed by a blow on the occiput; the cavity of the chest was instantly laid open, and the pericardium inspected. A small quantity of serum was found in it, which was removed with a sponge, and the incisions made were closed by sutures. At the end of twenty-four hours the sutures were divided, and the pericardium was again examined. Not a single drop of fluid had collected in it, in any instance, though in two of the trials the right auricle and ventricle were considerably distended with blood.

If these results be conclusive against fluid being effused into the pericardium after death in dogs, the conclusion from them, admits of being extended by analogy to other cavities of the same texture, and to man; and I am not acquainted with any pathological observations in opposition to it. The discovery of serous effusions in examinations *post mortem*, no symptoms of their occurrence or existence having been noticed during life, is surely no evidence of their having taken place after the cessation of vital action. It is too well known to be insisted on, that large portions of fluid may accumulate in the pericardium, and even in the ventricles of the brain, without a single symptom to indicate the fact.

Fort Pitt, Chatham,
March 5, 1822.

XXII. *On the mechanism of the Spine.* By HENRY EARLE, *Esq. F. R. S. Surgeon to the Foundling, and Assistant Surgeon to St. Bartholomew's Hospital.*

Read April 25, 1822.

HAVING been lately engaged in examining the structure of the vertebræ in different animals, I have been particularly struck with the mechanism of the spine and spinal canal in birds, by which a most remarkable degree of motion is gained in the neck, without any injury or pressure on a part of such vital importance to the existence of animal life, as the spinal marrow; an extent of motion, so great indeed as completely to compensate for the deficiency of it in the dorsal and lumbar regions, as well as for the want of any prehensile power in the anterior extremities. In attempting to explain the nature of this peculiar mechanism, which tends to throw considerable light on the physiology and pathology of the spine, I believe that I have not been preceded by any author. The cervical vertebræ in birds are very numerous, varying from nine to twenty-four.* They differ considerably from one another, according to their situation, in the form and direction of their articulating surfaces, and in the number and shape of

* This great diversity in the number of the cervical vertebræ in birds, is the more remarkable, when contrasted with the uniformity which pervades the class mammalia, where the number (with one single exception, the three-toed sloth) is constantly seven. The mole, whose head appears lost between the scapulæ, has precisely the same number as the giraffe and the horse.

the different processes, which afford extensive means of attachment to the muscles concerned in the different motions of the neck. Unlike the vertebræ in man and most of the mammalia, they are articulated together by complicated joints, which bear a close resemblance to the articulation of the olecranon with the humerus in the human subject, but differing in some respects; the vertebræ in birds admitting of lateral motion as well as flexion and extension, whilst the elbow is strictly a hinge-like joint.

The varying position of these articular surfaces is greatly favoured by the interposition of a cartilage, which is curiously adapted to the surface of each bone, and is enclosed between reduplications of synovial membrane; and thus each joint is double, consisting of two synovial cavities, and is analogous to the articulation of the lower jaw in man; a circumstance, I believe, not mentioned either by CUVIER, BLUMENBACH, or MACARTNEY.

The canal of each vertebra is of very unequal calibre, the centre being narrowest. It enlarges above and below, and at each joint is nearly three times the capacity that it is in the centre; and thus the canal of each individual vertebra may not unaptly be compared to an hour-glass. The canal is closed in front by the posterior surfaces of the bodies of the vertebræ, but behind it is very imperfect; and in the skeleton there is a large lozenge-shaped opening, formed by the diverging inferior articular processes and the converging plates which unite to form the back of the canal. This, in a recent state, is filled up by a membrane, and is protected by the highly elastic and powerful ligamentum nuchæ.

This mechanism, besides allowing of the greatest possible

freedom of motion, appears to be intended, at the same time, to guard against the possibility of any undue pressure on the spinal marrow. This is very readily demonstrated by removing the ligamentum nuchæ and membrane which closes the above mentioned opening. The spinal marrow, enveloped in its membranes, will immediately come in view. Its outer membrane is very vascular, and of a delicate structure, and is connected with the canal by a fine filamentous cellular substance; it is larger than the inner membrane, with which it is but loosely connected.

When the spinal canal has been thus exposed, the individual vertebræ may be bent backward to a right angle, and laterally to an angle of 45°, without in the least compressing the marrow which occupies so small a space of the whole calibre of the canal at each articulation, as to be quite secured from any injury from this motion. The design, in this structure, becomes even yet more obvious, on viewing the whole extent of the spinal cord. It is nearly of the same size throughout, diminishing very gradually from above downwards, and completely occupies the narrow central part of the canal of each cervical vertebra, where no motion can affect it. The same may be observed in that part of the spine which corresponds to the dorsal and lumbar divisions, which in birds do not admit of motion; for here we find no variation, either in the spinal canal or the marrow, except where the numerous branches are given off to form the great sciatic plexus, to supply the lower extremities, where it swells out into a bulbous shape, corresponding to the cavity in the bone.

Before quitting the subject of the spine in birds, it will be

right to mention one more peculiarity, apparently connected with the same mechanism. Contrary to the usual course in other animals, the nerves that are given off from the cervical portion of the spinal marrow, pass obliquely upwards at a considerable angle, through an opening between the root of the inferior articulating process, and the body of the bone; they then divide, and one branch descends through the opening in the lateral process, and the other branch is distributed to the surrounding muscles and integuments.

One principal object in comparative anatomy, or rather in comparative physiology, is to enable us, by examining particular structures, which are more developed in some animals, and in whom consequently the functions of such structures are more apparent, to judge of the probable uses of similar structures existing in a diminished proportion in other animals. On investigating this subject, and examining the spines of several other animals, I have found a similar arrangement, varying only in degree, and that exactly in proportion to the extent of motion permitted between the vertebræ.*

In the formation of the spine in man, it was requisite to combine two very opposite qualities. The solidity and strength of a column were required to be united with the flexibility necessary to the performance of our diversified actions. To attain

* This rule will be found to hold good, even in those animals which form exceptions, with respect to the general form and arrangement of the different processes. Thus, in the mole, whose cervical vertebræ are mere bony rings without any spinous processes, and which, consequently, admit of extensive motion, the canal is remarkably capacious. In the bat, whose dorsal vertebræ are either wholly without spinous processes, or have only short tubercles, the canal, at this part, is of greater volume than either in the cervical or lumbar vertebræ; and, contrary to the general rule, this division of the spine admits of considerable motion.

these various ends, this beautiful structure is admirably adapted. The broad horizontal planes afforded by the bodies of the vertebræ, the mechanical locking of the articular processes, and the powerful ligamentous bands which unite them, so connect the whole as to form one column, whilst the numerous articulations into which it is subdivided, which are separated by masses of highly elastic matter, at once interrupt the effect of concussion, and allow a slight extent of yielding of one vertebra upon the other. The motions of the individual vertebra are obscure and limited, but the aggregate of the whole is considerable. The extent of motion varies in each region; in the back, every thing conspires to limit it; but in the neck and loins it is much greater; and in sawing open the spinal canal, we find a very similar provision to that which I have before described in birds, namely, an exact correspondence between the extent of motion permitted, and the size and form of the canal. Thus, in the dorsal division, where motion hardly exists, its calibre is less; it is of a rounded form, and it is more closely adapted to the size of its contents. In the superior cervical vertebræ, where the extent of motion is greater, the canal is of a triangular form, and is considerably larger in proportion to the spinal cord. In the lumbar vertebræ it is also triangular, and much more capacious than in the dorsal. Obviously, with the same intention, the theca is very loosely connected with the bony canal, and a considerable space is left between it and the other membranes, to allow of a sufficient play of one surface on the other, so that at the greatest extent of natural curve, no perceptible stretching of the marrow can take place, which would be liable to continual pressure, if, closely enveloped in its

membranes, it completely filled the canal, in every motion of which it must, in that case, participate.

To afford additional support to the marrow which this loose state of membranes would leave very insecure, if enclosed in so delicate a tissue as the pia mater of the brain, this membrane, which may be considered as the proper tunic of the marrow, is greatly thickened, and partakes more of the characters of a fibrous membrane.

The membranous band, which has been termed the ligamentum denticulatum, appears to be superadded to restrain the lateral motions of the marrow, and to steady it in the canal. By these membranous processes, the marrow may not, improperly, be said to be lashed to the sides of the spinal sheath, in which, from the disproportion between them, it would otherwise be liable to perpetual variation of position, and pressure from the bony parietes. That a certain degree of freedom of motion between the membranes is essential to the due performance of the functions of the spinal marrow, is proved by the effect of accidents and disease. It would be out of place here, to bring forward a detail of particular cases, but I may mention briefly, that I have ascertained, by dissection, that the most distressing train of nervous symptoms, and even complete paraplegia, may be produced by adhesions taking place between the membranes, and by effusion into the canal or theca.

In conclusion I may observe, that this view of the subject tends to throw considerable light on the pathology of the spine, and assists in explaining a circumstance which I have repeatedly noticed in diseases affecting the vertebræ, namely, that the symptoms of irritation and inflammation of the spinal

marrow, are much more early manifested, and are generally far more serious in their consequences when the dorsal vertebræ are affected, than when either the cervical or lumbar are the seat of disease. In the former case, the slightest congestion or effusion is often productive of serious symptoms, from the canal being smaller and more completely filled with the marrow and its membranes; whilst, in the latter description of cases, from the greater capacity of the canal and looseness of the membranes, considerable effusion may exist, without, at first, producing any marked symptoms, more particularly in the lumbar region, where other circumstances concur in rendering the effect of pressure less sensibly felt; to enter into a description of which, would be foreign to the object of this paper.

As it is difficult to convey any clear idea of complicated forms by words, I have subjoined a sketch of some cervical vertebræ in birds, with a description of the different parts.

EXPLANATION OF PLATE XXX.

Figure 1. Represents an anterior view of a single cervical vertebra.

Fig. 2. A posterior view of the same.

Fig. 3. A lateral view of the same.

Fig. 4. A front view of two vertebræ articulated together.

Fig. 5. A back view of the same.

The letters of reference are the same in all the figures.

A. The body of the vertebra.

BB. lateral processes.

CC. Styloid processes, with tubercles near their base, which restrain the motion of the vertebræ in the anterior direction.

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5

Fig. 6.

In some birds, at the upper part of the neck, a bony arch extends from one to the other, to preserve the blood vessels which pass under it from pressure. This is the case in the heron, and other very voracious birds.

D. Lunated excavation for articulation with the superior vertebra.

E. Semi-lunar convexity, corresponding to the excavation above mentioned.

F F. Diverging inferior articular processes, with surfaces facing obliquely outward.

G G. Superior articular surfaces, facing obliquely inwards.

H. I. Spinal canal, imperfect in the skeleton, in consequence of the vacancy left between the diverging inferior articular processes and the superior converging plates, which unite to form the back of the canal K.

L. Opening for the passage of the spinal nerves.

M. Foramen for the passage of the carotid artery and branches of the spinal nerves.

Fig. 6. A perpendicular section of two vertebræ, showing the interarticular cartilages.

28, George Street, Hanover Square,
April 2, 1822.

XXIII. *Of the Nerves which associate the muscles of the Chest, in the actions of breathing, speaking, and expression. Being a continuation of the paper on the Structure and Functions of the Nerves.* By CHARLES BELL, *Esq.* *Communicated by* Sir HUMPHRY DAVY, *Bart. LL. D. P. R. S.*

Read May 2, 1822.

IN a former paper an examination was made of the nerves of the face; that part of the system was taken, as proving in a manner the least liable to exception, that two sets of nerves, hitherto undistinguished, possessed distinct powers; and that very different effects were produced when the muscles and integuments were deprived of the controuling influence of the one or of the other of these nerves. In that paper it was shown, that parts remote in situation, were yet united by the closest sympathy with the lungs. That by a division of one nerve, these organs could be severed from the other parts of the apparatus of respiration; and though rendered dead to the influence of the heart and lungs, were yet possessed of their other properties, such as sensibility and voluntary motion.

In the present paper it is proposed to prosecute this subject, by tracing the nerves which influence the motions of the trunk of the body in respiration, and to subject them to a similar enquiry.

It is an encouraging circumstance to the Author of this Paper, and may incline the Society to bear with the detail

into which it will be necessary to enter, that already practical benefits have arisen from the former paper; that the views presented there, as connected with general science, being carried into practice, have enabled the physician to make more accurate distinctions of disease, and the surgeon, in removing deformity, to avoid producing distortion.

Of the motions of the thorax, as affording a key to the intricacy of its nerves.

We have seen the necessity of considering all the functions and relations of a part of the animal machine, the nerves of which we propose to distinguish according to their uses; and this is even more necessary with respect to the thorax than the face. This will be evident, if we make a mere catalogue of the uses of this compages of bones and muscles. Besides affording support and protection to the heart and lungs, and the viscera of the higher region of the abdomen, the thorax performs these offices:

1. It alternately opposes and yields to the weight of the atmosphere, thus producing respiration.

2. In addition to the uniform motion of the chest in breathing, there is the occasional increase and agitation commensurate to the excited state of the animal frame, when additional muscles are brought into action.

3. There is the exertion of the respiratory apparatus in natural voice, and in articulate language.

4. Through the nerves and muscles employed in respiration, are also exhibited the emotions and passions of the mind.

5. The organs of the sense of smelling, and particularly the muscles which move the cartilages of the nose, are, in their exercise, as necessarily joined to the act of inspiration, as those of speech are to the act of expiration.

6. The powers of the arms in voluntary exertion, are in a great measure dependent upon the expansion of the thorax; so that the act of inspiration is always combined with sudden and powerful exertion. The more indeed we attend to the motions of the frame, whether in efforts of strength, or in the act of respiration, the more remarkable will the unexpected combinations of the muscles appear.

It is only when we are made sensible of the extent of the respiratory actions, and that they in effect extend over the whole face and neck and trunk, that we can comprehend how the mechanism of the thorax, or rather of the respiratory apparatus generally, affects the arrangement of the whole nervous system. Wherever, in examining the comparative anatomy of animals, we find ribs rising and falling by respiratory muscles, we have a *medulla spinalis*, and the distinction of *cerebrum* and *cerebellum*. And experiment and observation prove, that the seat of that power which controuls the extended act of respiration, is in the lateral portions of the *medulla oblongata*, from which it is continued through certain respiratory nerves which pass out from the neck, and also downwards, by corresponding columns of the spinal marrow, to the intercostal nerves.

Origins of the respiratory nerves.

The nerves on which the associated actions of respiration depend, and which have been proved to belong to this system, by

direct experiment, and the induction from anatomy, arise very nearly together. Their origins are not in a bundle, or fasciculus, but in a line or series, and from a distinct column of the spinal marrow. Behind the *corpus olivare*, and anterior to that process which descends from the cerebellum, the *corpus restiforme*, a convex strip of medullary matter, may be observed, and this convexity, or fasciculus, or *virga*, may be traced down the spinal marrow, betwixt the sulci, which give rise to the anterior and posterior roots of the spinal nerves.

This portion of medullary matter is narrow above where the *pons Varollii* overhangs it. It expands as it descends; opposite to the lower part of the *corpus olivare* it has reached its utmost convexity, after which it contracts a little, and is continued down the lateral part of the spinal marrow.

From this track of medullary matter on the side of the *medulla oblongata*, arise in succession from above downwards, the *portio dura* of the seventh nerve: the *glosso-pharyngeus* nerve: the nerve of the *par vagum*: the *nervus ad par vagum accessorius*: the *phrenic*, and the *external* respiratory nerves.

It is probable that the branches of the intercostal and lumbar nerves, which influence the intercostal muscles and the muscles of the abdomen in the act of respiration, are derived from the continuation of the same cord or slip of medullary matter. Nor will it escape observation, that the nerves called phrenic and external respiratory, though coming out with the cervical nerves, do, in all probability, take their origin from the same portion of the medulla spinalis with the accessory nerve.

The intercostal nerves, by their relations with the medulla oblongata, are equal to the performance of respiration, as it

regards the office of the lungs; but they are not adequate to those additional functions which are in a manner imposed upon the respiratory apparatus, when they are brought to combine in other offices.

Of the muscles of the trunk, which are brought in aid of the common respiratory muscles.

If we look upon the frame of the body for the purpose of determining which are the muscles best calculated to assist in the motions of the chest, when there is an increased or excited action, we shall have little difficulty in distinguishing them, and we shall have as little hesitation in assigning a use to the nerves which supply these muscles exclusively. For these nerves have the same origin: they take an intricate course, threading and passing betwixt other nerves and other muscles, to be entirely given to the muscles which heave the chest.

In this enquiry it is necessary to observe, that the life of animals is protected by a particular sense which gives rise to an instinctive motion of drawing the breath, and by which the chest is suddenly and powerfully expanded on exertion or alarm. The start on sudden alarm, is accompanied with a rapid expansion and rising of the chest, and the voice, at such a moment, is produced by suddenly inhaling, and not by expiration; and this expansion of the chest combines with the preparation for flight or defence, since the extension of the muscles lying on the breast and back is produced by this motion, and since they are thereby rendered more powerful in their influence upon the arms or anterior extremities. It cannot escape observation, that oppression and difficulty of

breathing is exhibited in gasping and forcible inspiration, in drawing the breath, not in throwing it out.

Accordingly, when we examine the trunk of the human body, we have no difficulty in distinguishing the muscles most capable of raising the chest; and these in effect, we see powerfully influenced in deep inspiration, whether the action be voluntary, as in speech, or involuntary, as in the last efforts of life, when sense is lost. They are the mastoid muscle, the trapezius, the serratus magnus, and the diaphragm.

1. *Sterno-cleido-mastoideus.** This muscle, by its attachment to the sternum or breast bone, raises or heaves the chest; and the operation of this muscle is very evident in all excited states of respiration, in speaking, and still more in singing, coughing, and sneezing. But there is something necessary to the full effect of this muscle on the chest, for otherwise it will be a muscle of the head, and not of the chest.

2. *The trapezius* † must fix the head or pull it backwards before the *mastoideus* can act as a respiratory muscle, and how they are combined we shall presently see. The position of the head of the asthmatic, during the fit, as well as the posture of the wounded or the dying, prove the influence of the upper part of the trapezius in excited respiration.

The trapezius has a still more powerful and important influence in respiration when the action rises above the ordinary condition, and that is by drawing back the scapula, to give the necessary effect to the action of the serratus magnus.

3. The *serratus magnus anticus* ‡ being extended over the whole side of the chest, and attached in all the extent from the second to the eighth rib, is very powerful in raising the

* See Plate XXXI. fig. 2, A. † Fig. 2, B. B. ‡ Fig. 2, E. E.

ribs; but it cannot exert this power, independently of the trapezius, since, without this combination, its force would be exerted in moving the scapula, and not the ribs; unless the scapula be fixed, or pulled back by the *trapezius*, the *serratus* is not a muscle of respiration.

In this manner do these three powerful muscles hang together in their action, combining with the diaphragm to enlarge the cavity of the chest in all its diameters.

The course of our enquiry leads us to ask, are these muscles privileged above others by any peculiarity of nerves? And the answer is plain: to these muscles alone, are the nerves, which I am about to call respiratory nerves of the chest, distributed.

*Anatomy of the respiratory nerves of the trunk.**

The nerves which give rise to the extraordinary intricacy of this system on the side of the neck, are the spinal accessory nerve, the phrenic nerve, and the external thoracic nerve. By reference to any common book of anatomy, the phrenic nerve (4 Fig. 2.) will be found to have its great root or origin from the fourth cervical nerve; and there joins this, a more slender branch from the third cervical nerve. But, besides these roots, it has connections, which of themselves would mark the relations of the nerve; high in the neck, it is connected with the *nervus vagus* and with the *lingualis medius*, while, at the same time, a branch is given off to the muscles of the larynx. The trunk of the nerve descends into the cavity of the thorax, and gives no branches until arriving at the diaphragm (Fig. 2.G), it sends out numerous diverging branches, which are lost in the substance of that muscle.

* See Plate XXXI. Fig. 1 and 2.

It has been long known that irritation of this nerve convulses the diaphragm, and that cutting it across paralyses that muscle. These facts, with the consideration of its course, prove it to be a respiratory nerve, and such has been the universal opinion.

But to what purpose should a distinct nerve be sent to the diaphragm, if the other muscles, seated externally, and which are associated in action with the diaphragm, and as important to respiration, were left without a similar tie to unite them with each other, and with the organs of the voice?

The external respiratory nerve of the thorax (5 Fig. II.) is a counterpart of the internal or phrenic nerve. It comes out from the 4th and 5th cervical nerves, and often it is connected with the phrenic. It diverges somewhat from that nerve, because, instead of descending within the chest, it falls over the ribs, and descends in a distinct flat trunk upon the outside of the chest, to be distributed intirely to the *serratus magnus anticus*. This muscle has nerves from the spinal marrow, because it has to combine in the motions of the frame in loco-motion. But the long descending nerve is a respiratory nerve; which we may know from its origin, course, and destination; in its origin and course it is like the diaphragmatic nerve, and in its destination also, since it is given to a muscle necessary to full inspiration.

I come now to the *spinal accessory nerve* (Plate XXXI. fig. II. 8)* which is more particularly an object in this paper. It is called here the superior respiratory nerve of the trunk. Experiments may take a colour from the preconceived idea, but the accurate investigation of the structure will not deceive

* *Nervus ad par vagum accessorius.*

us. The author therefore entreats attention to the anatomy of this nerve, as leading in the most conclusive manner to a knowledge of its functions.

It arises from the cervical portion of the spinal marrow (Plate XXXI. fig. I. 4); but instead of collecting its branches to go out by the side of the vertebræ, like the internal and external respiratory nerves, it shoots upwards through the theca of the spinal marrow, enters the skull, and joins the 8th pair of nerves; from which it has its term of accessory. We see the roots of this nerve as far down as the 4th cervical nerve.* These roots arise neither from the posterior nor the anterior column of the spinal marrow, but betwixt the posterior roots of the cervical nerves and the *ligamentum denticulatum*, and from the *column of medullary matter* above described. The origins of this nerve come off in one line, and that line is in the direction of the roots of the eighth pair, and of that nerve which has been proved to be the respiratory nerve of the face. In its ascent the accessory nerve is attached to the posterior root of the first cervical nerve.

The nerve having ascended through the *foramen magnum*, passes out from the skull associated with the nerves constituting the *eighth pair*, and in the same sheath with them; they all go out through the *foramen lacerum*, and by the side of the jugular vein. In this course the accessory nerve divides into two. One of these divisions joins filaments of the *par vagum* (Fig. II. 6); and these again send nerves to the *glosso-pharyngeal* nerve (Fig. II. 2); and sometimes a branch may be seen going to the *lingualis medius*. The

* In the ass, its roots are seen to extend much lower down.

more exterior division of the accessory nerve descends behind the jugular vein, and comes forward and perforates the mastoid muscle, (Plate XXXI. fig. II. A). In its passage through the muscle it sends off branches which course through its substance; and if, as sometimes happens, though rarely, the nerve does not pass through the muscle, these branches are, notwithstanding, invariably given to it.

When the nerve has escaped from the back part of the mastoid muscle, it forms a communication with that branch of the 3d cervical nerve that ascends behind the muscle; and nearly at the same time it is joined by a branch from the 2d cervical nerve. The superior respiratory nerve now descends upon the neck, and begins to disperse its branches in regular order to the edge of the trapezius muscle; (Fig. II. B.) four or five branches take their course to that muscle, separate into minute subdivisions, and are lost in its substance. One more considerable division, being the lowest of these, is joined by a long descending branch of the 2d cervical nerve. Encreased by this addition, it descends under the trapezius and behind the clavicle. Following this descending branch, it will be found exclusively attached to the trapezius. Behind the scapula it is again joined by branches from the spinal nerves; and here a sort of imperfect plexus is formed, from which divisions of the nerve, still descending, follow the lower edge of the muscle, and are finally dispersed among its fibres.

This nerve arises from the same column with the respiratory nerves; it takes a most intricate and circuitous passage to form a junction with nerves which we know to belong to that class; it sends branches to join the nerves of the tongue

and pharynx; it sends branches to the larynx in company with the branches of the *par vagum*; it then crosses the great nerves of the neck, passes under the spinal nerves, goes to no other muscles in its course, but lavishes all its branches on the mastoid and trapezius muscles. To an anatomist it is as plainly set forth as if it were written in our mother tongue, this is *the superior respiratory nerve of the trunk.**

Comparative view of these nerves.

If we examine the *par vagum*, the *portio dura* of the face, the *external thoracic*, the *diaphragmatic*, and the *spinal accessory* nerves, by comparative anatomy, we shall conclude that they are all respiratory nerves, by their accommodating themselves to the form and play of the organs of respiration. In fishes, the respiratory nerve† goes out from the back part of the *medulla oblongata*. When it escapes from the skull it becomes remarkably enlarged, and then disperses its branches to the branchiæ and the stomach. But from the same nerve go off branches to the muscles moving the gills and operculum, whilst a division of the nerve is prolonged under the lateral line of the body to the tail. It is said, this division sends off no branches, but this is not correct; it gives branches in regular succession to the muscles from the shoulder to the tail. Experiments have been made upon these nerves, but their detail would lead us too far. It is scarcely necessary to add, that there is neither phrenic nor spinal accessory, nor

* *Lobstein*, in a dissertation on this nerve, finding the difficulty of accounting for the *nervous fluid* coming by a double passage to the muscle, concludes, *veniet forsan tempus quo ista quæ nunc latent, dies extrahat et longioris ævi diligentia.*

† The nerve which by its subdivision supplies the heart, lungs, and stomach, and the muscles of the gills.

external thoracic nerves in fishes, the order of their muscular system not requiring them. In birds, the structure of the wing, and the absence of the mastoid muscle, render the spinal accessory nerve unnecessary; it is wanting for the reason that in the absence of the diaphragm there is no phrenic nerve. Quadrupeds have the three respiratory nerves of the trunk; but even in them there are variations in the muscular frame, which illustrate the appropriation of the nerves. The construction of the neck of the camel is like that of birds; there is a succession of short muscles along the side of the neck, and attached to the vertebræ; but there is no long muscle, like the *sterno-cleido-mastoideus*, contributing to the motion of respiration. There is, accordingly, no spinal accessory nerve in the neck of this animal.

We have a remarkable example of the manner in which these nerves vary in their course of distribution, and yet retain their appropriate functions, in the nerves of the neck of birds. In them, the bill precludes the necessity of the portio dura going forward to the nostrils and lips; the nerve turns backwards, and is given to the neck and throat; and it is particularly worthy of remark, that the action of raising the feathers of the neck, as when the game cock is facing his opponent, is taken away by the division of this nerve.

The functions of these nerves farther illustrated.

Before having recourse to experiments on brutes, we may observe what takes place in our own bodies By placing the hand upon the neck, we may be sensible that the mastoid muscle has two motions. The lower extremity of the muscle is fixed when we move the head; but when we use the muscle

in inspiration, the head, and consequently the upper extremity of the muscle, are fixed. Now, if we endeavour to raise the sternum through the operation of this muscle, we shall find that other muscles are, insensibly to us, brought into action, which have nothing to do with this raising of the sternum. For example; if we strain to raise the lower extremity of the muscle, we shall unavoidably produce an action of the muscles of the nostrils; by which association of actions, we shall discover, that we are using the *mastoideus* as a respiratory muscle. If we reverse the action, and move the upper extremity of the muscle, other muscles will be drawn into co-operation, but they will be such as assist in the motion given to the head. Or we may vary the operation in another way. In snuffing or smelling, if we place the fingers on the portions of the mastoid muscles which are attached to the sternum, we shall find every little motion of the nostrils accompanied with corresponding actions of the sternal portions of the muscles in the neck.

A man having a complete hemiplegia, the side of his face relaxed, the arm hanging down powerless, and the leg dragged in walking, we were curious to know if the influence pervaded all the nerves of the side, or only the regular or voluntary nerves. Some trouble was taken to make him heave up the shoulder of the debilitated side, but to no purpose. He could only do it by bending the spine to the other side, and as it were weighing up the paralytic shoulder. But on setting him fairly in front, and asking him to make a full inspiration, both shoulders were elevated at the same time that both the nostrils were in motion. The respiratory nerve of the face, and the superior respiratory nerve, were entire

in their office; and, although the regular system of nerves refused acting, the *sterno-mastoideus* and the *trapezius* partook of their share in the act of respiration. Seeing that the mastoid muscle has two sets of nerves, that one of these is of the class of voluntary nerves, and the other of respiratory nerves, are we not borne out in concluding, that when the head is moved, being a voluntary act strictly, it is performed through the common class of voluntary nerves? that when the chest is raised, it is an act of respiration, and is affected through those nerves which controul the muscles in respiration?

This conclusion is confirmed by the following experiment. In the ass, there are two muscles which take the office of the mastoid muscle; one is inserted into the jaw, which we may call *sterno-maxillaris*, and the other into the vertebræ, viz. *sterno-vertebralis*. To these the superior respiratory nerve (or spinal accessory) is distributed in its passage to the trapezius. These muscles are at the same time supplied with numerous nerves directly from the spinal marrow. If we expose the superior respiratory nerve, and then induce excited respiration, so as to bring these muscles into powerful action in combination with the other muscles of respiration, and if, while this action is performed, we divide the nerve, the motion ceases, and the muscle remains relaxed until the animal brings it into action as a voluntary muscle.

An ass being thrown, its phrenic nerves were divided, on which a remarkable heaving of the chest took place. It rose higher, and the margins of the chest were more expanded at each inspiration. There was no particular excitement of the muscles of the neck, shoulder, or throat, at this time; so

that to excite the actions of these muscles, it was necessary to compress the nostrils. When they began to act with more violence, keeping time with the actions of the other muscles of respiration, the superior respiratory nerve was divided; immediately the action ceased in the muscles attached to the sternum of the side where the nerve was divided, while the corresponding muscles of the other side continued their actions.

After dividing the spinal marrow between the vertebræ of the neck and those of the back, respiration is continued by the diaphragm: which experiment, as it is often mentioned by physiologists, the author has not thought it necessary to repeat, but only to institute the following experiment on an ass. The phrenic nerves being first divided, and then the spinal marrow cut across at the bottom of the cervical vertebræ, respiration was stopt in the chest; but there continued a catching and strong action at regular intervals in the muscles of the nostrils, face, and side of the neck. The main part of the apparatus of respiration was stopped, but these accessory muscles remained animated, and making ineffectual endeavours to perform the respiration. When apparent death had taken place, the ass was re-animated by artificial breathing, and then these muscles on the face and neck were restored to activity, and became subject to regular and successive contractions, as in excited respiration, whilst the chest remained at rest. These actions continued for a short time, and then ceased; but upon artificial respiration being again produced, the same results followed. This was repeated several times, the animal remaining insensible during these experiments.

Upon stimulating the nerves after the death of this animal, it was observed, that the class of respiratory nerves retained their power of exciting their respective muscles into action, long after the other nerves had ceased to exert any power; they were evidently of that class which retain their life the longest.

It is a duty to avoid the unnecessary repetition of experiments, and I have now to make a short statement of facts, resting on the highest authorities: experiments made without reference to the views now presented to the Society.

The division of the recurrent branch of the *par vagum* destroys the voice.*

The division of the laryngeal branch of the *par vagum* stops the consent of motion between the muscles of the *glottis* and the muscles of the chest.†

The injury or compression of the *par vagum* produces difficulty of breathing.‡

By the assistance of these well known facts, we complete the knowledge of the circle of actions which result from the respiratory nerves.

The *medulla oblongata* and *spinalis* are composed of columns of nervous matter, which (from the different powers of the nerves, as they arise from the one or other of these columns), possess distinct properties. In animals that breathe by ribs and a numerous class of muscles, and which animals have a spinal marrow, we see that a column of nervous

* Sectis ambobus nervis recurrentibus vox perit: *Arnemann, Sömmerring, Morgagni.*

† *Le Gallois.*

‡ Vinculo compressis nervis vagis oriuntur in bestiis spirandi difficultas, surditas, vomitus, corruptio ciborum in ventriculo. *Sömmerring, Haller, Brun de ligaturis nervorum.*

matter is embraced between the anterior and posterior *virgæ* of that body, and that this portion may be traced downwards between the roots of the spinal nerves. From the upper part of this column, where it begins in the *medulla oblongata*, the several nerves proceed which have formed the subject of these papers, and on the influence of which, it has been proved, the motions of respiration principally depend. It is not an extravagant conclusion to say farther, that the power of the regular succession of intercostal and lumbar nerves, as far as they regulate the respiratory actions, proceeds from the connections of the roots of these nerves with this column, which is continued downwards, and which can throughout be distinguished from the rest of the spinal marrow.

We are now enabled to distinguish the influence of the spinal marrow and its regular succession of nerves, from those which have been traced in these papers. The first are essential to the act of respiration; without them the others are unequal to the task. But on the other hand, although the regular succession of spinal nerves be equal to the raising and depressing the thorax, they are not competent to the performance of the motions of the glottis, pharynx, lips, and nostrils, which several parts are necessarily influenced in excited respiration, as well as in the acts of smelling, coughing, sneezing, and speaking: for these, the co-operation of the whole extended class of respiratory nerves is required.

Surveying the complicated machinery which in man is prepared for these various offices, we may reap the benefit of these fatiguing details, in the contemplation of the most interesting phenomena in nature. The relations of the subject

may be presented under the heads of pathology, and expression.

Of pathology, as illustrated by a knowledge of the respiratory system of nerves.

When we survey the full extent of the respiratory system of nerves, we are prepared to comprehend its importance to the continuance of life. The infant born without a brain can breathe if the origins of these nerves be entire. Deep wounds of the brain, though eventually fatal, are not necessarily, or instantly so. The man wounded in the spine below the origins of the nerves which we have traced, drags on existence for a time; but a bruise on the part of the *medulla oblongata*, from which these nerves take their departure, is death in the instant; a breath is not drawn again.

In describing the effects of violence on the medulla oblongata, authors have attributed the sudden death to injury of the roots of the nerves of the par vagum; and yet we have a statement from the same authority, that an animal will survive the division of both nerves of the par vagum. Now that we find that many respiratory nerves depart from the same centre, and go out to all the parts of the muscular frame, which move in respiration, we can better comprehend, how injury of the medulla oblongata suppresses at once the act of respiration in the nostrils, throat, and windpipe, and the action of the muscles both without and within the chest; even the expression of the agony of dying is, by the injury of the roots of all these nerves, suddenly interrupted, and actual death follows quickly, owing to the cessation of the respiratory functions.

A young man was brought into the Middlesex Hospital, who had fallen upon his head. He soon recovered, and lay for some time in the hospital without exhibiting a symptom to raise alarm. He had given thanks to the assembled Governors of the Hospital, and had returned into the ward for his bundle, when, on turning round to bid adieu to the other patients, he fell, and in the instant expired. Upon examining his head, it was found that the margins of the occipital hole had been broken: no doubt it had happened that in turning his head the pieces were displaced, and closed and crushed the medulla oblongata, as it passes from the skull.

A man was trundling a wheel-barrow in Goodge Street, which is immediately adjoining the Middlesex Hospital: in going from the carriage-way to the flag-stones he met the impediment of the curb-stone. He made several efforts to overcome it, and at length drawing back the wheelbarrow, he made a push, and succeeded; but the wheel running forward, he fell, and remained motionless. He was taken into the Hospital, but he was found to be quite dead. The tooth-like process of the second vertebra of the neck had burst from the transverse ligament of the first.* The impulse given to the head had done this violence, and had at the same time carried forward the spinal marrow against the process, and on which it was crushed.

We have seen by experiments, that the respiratory nerves are distinguished from the other nerves by retaining their power longer: that they are alive to impression, and can be

* In my collection there is a preparation which exhibits this ligament destroyed by disease. The death was sudden, and caused by the falling forward of the head, and the crushing of the medulla spinalis.

made to produce convulsions in the muscles they supply, after the other nerves are dead to the application of stimuli. In disease, during the oppression of the mental faculties, and on the approach of death, we witness these nerves, and the muscles put into operation by them, continuing their functions, when in other respects the body is dead. This circumstance, so familiar to the medical observer, might have led to the conclusion to which we have arrived, more laboriously, through anatomical investigations: that there are a great many muscles extended over the body, and which perform the common offices under the will, which are occasionally drawn into combination with the muscles of respiration, and are held in relation to the vital functions by a distinct system of nerves, and that these nerves have a centre and a source of power, different from that of the voluntary nerves.

These nerves, so peculiar in relation and function, are differently influenced by disease from the other division of the nervous system. Their functions are left entire when the voluntary nerves have ceased to act, and they are sometimes strangely disordered, while the mind is entire in all its offices, and the voluntary operations perfect. In tetanus the voluntary nerves are under influence, and the voluntary motions locked up in convulsions; in hydrophobia the respiratory system is affected; and hence the convulsions of the throat, the paroxysms of suffocation, the speechless agony, and the excess of expression in the whole frame, while the voluntary motions are free.

The confusion between vital and voluntary nerves, the combining the par vagum and sympathetic nerves together, and the exclusion of the *portio dura* of the 7th nerve, the spinal accessory nerve, and the external thoracic nerve, from their

natural classification with the diaphragmatic or phrenic, has given rise to very vague theories, and occasioned very inaccurate statements of pathological facts. What remains to be said under this head, I would rather offer in the form of queries.

The frequency of sudden death, where no corresponding appearances are exhibited in the brain or heart, leads us to consider more attentively the only part of the system through which life can be directly extinguished. In *angina pectoris*, we witness the agony of suffering in this system when the patient survives; and when he dies suddenly, we can imagine it to proceed from an influence extending over these nerves, and interrupting the vital operations. We have seen that a branch of this system may suddenly cease to operate on the corresponding muscles, and that in this way the side of the face may be deprived of all participation in the act of respiration, and all expression be lost. What would result from a more universal defect in the actions of this class of nerves, but sudden death?

Could we expect that the diseases of lethargy and somnolency, should be distinctly divided from apoplexies, while the organs on which the distinction of symptoms principally depend, were imperfectly understood?

The stomach, supplied with the great central nerve of this system, exhibits the most powerful influence on these extended nerves; a blow on the stomach "doubles up" the bruiser, and occasions that gasping and crowing which sufficiently indicates the course of the injury; a little more severe, and the blow is instantly fatal.

The position of the asthmatic, shows how this system is

affected; whether directly or indirectly, it is not our present business to enquire. He stands stooping forward, resting his arms so as to throw the muscles of the chest into operation upon the ribs. The position of the head and the rigidity of the muscles of the neck, the action of the mastoid muscle, and of the cutaneous muscle, visible in the retraction of the cheeks and mouth, and the inflation of the nostrils, carry us back in review of the nerves and muscles of respiration.

It will now perhaps be acknowledged, that the methods of physiologists, in accounting for the combination of parts in the actions of respiration, were very imperfect, or rather altogether erroneous. To account for the convulsion of the diaphragm in sneezing, they were constrained to go a far way about: first, connecting the roots of the phrenic with the sympathetic nerve: bestowing sensibility on the latter, which it does not possess: then, following a remote connection between it and the nerves of the nose; then again, counting the relations between the facial nerve and the 3rd of the neck: they satisfied themselves that they had explained the manner in which the diaphragm became convulsed upon irritating the membrane of the nose. Another misconception was engrafted on the first; they spoke of these actions as convulsive and irregular, which are amongst the most admirable provisions for the protection of life. As to the act of sneezing, like coughing, it is a consequence of an irritation of the extremity of one of the respiratory nerves, whence the whole muscles of respiration are brought into action. That there is nothing accidental, nor of the nature of convulsion, is shown, by the admirable adjustment of the muscles to the object. A body irritating the glottis, will call into simultaneous action the

muscles of respiration, so as to throw out the air with a force capable of removing the offending body. But if the irritation be on the membrane of the nose, the stream of air is directed differently, and, by the action of sneezing, the irritating particles are removed from these surfaces. By the consideration of how many little muscles require adjustment to produce this change in the direction of the stream of air, we may know, that the action is instinctive, ordered with the utmost accuracy, and very different from convulsion.

We may notice another office of these nerves; in smiling, laughing, and weeping, the influence is solely propagated through them. The face we have seen is dead to all changes of the kind when the nerve of this class which goes to it is destroyed, whether it be by division of the nerve, or from its being surrounded with inflammation or suppuration. When we consider that all the respiratory nerves depart from the same source, and participate in the same functions; and more especially when we see the respiratory organs so very distinctly affected in the conditions of the mind, which give rise to these affections, it is not too much to suppose, that what is proved in regard to one of these nerves, is true of the whole class, and that they alone are influenced in laughter. Physiologists who have not investigated the cause, are yet agreed in describing laughter to be a condition of the respiratory muscles, where the air is drawn in rapidly, and thrown out in short spasmodic motions of these muscles; that crying is nearly the reverse, the inspiration being cut by spasmodic actions of the muscles of inspiration. By these considerations are explained the *subrisus* which arises from abdominal irritation, and the sardonic retraction of the muscles of the face produced by

wounds of vital parts, and particularly of the diaphragm. It explains also the successive convulsive lifting of the shoulders in wounds of the diaphragm.

That a system of nerves so intimately combined as this is with the other parts of the general system, should suffer in hysterical disorders, cannot surprise us; and admitting that irritation reaches to the respiratory system, we may perceive how rapidly the change may be produced, from the convulsions of laughter to those of crying; and where, if there be a corresponding condition of the mind, it rather follows than precedes the expression of the frame.

These respiratory nerves are organs of expression.

It would have been extraordinary if we had arrived at any satisfactory theory of expression, before it was known through what instruments the mind influenced the body, during emotion or passion. But since we know that the division of the respiratory nerve of the face, deprives an animal of all expression; and that the expressive smile of the human face is lost by an injury of this nerve: since it is equally apparent, that the convulsions of laughter arise from an influence extended over this class of nerves; it comes to be in some sort a duty in pursuing this matter, to examine farther into the subject of expression. We may be at the same time assured of this, that whatever serves to explain the constant and natural operations of the frame, will also exhibit to us the symptoms of disease with more precision.

In terror, we can readily conceive, why a man stands with eyes intently fixed on the object of his fears: the eyebrows elevated, and the eye balls largely uncovered; or why,

with hesitating and bewildered steps, his eyes are rapidly and wildly in search of something. In this we only perceive the intent application of his mind to the objects of his apprehensions, and its direct influence on the outward organs. But when we observe him farther, there is a spasm on his breast: he cannot breathe freely: the chest remains elevated, and his respiration is short and rapid: there is a gasping and convulsive motion of his lips: a tremor on his hollow cheeks: a gulping and catching of his throat: his heart knocks at his ribs, while yet there is no force in the circulation, the lips and cheeks being ashy pale.

It is obvious that there is here a reflected influence in operation. The language and sentiments of every people have pointed to the heart, as the seat of passion, and every individual must have felt its truth. For though the heart be not in the proper sense the seat of passion, it is influenced by the conditions of the mind, and from thence its influence is extended through the respiratory organs, so as to mount to the throat, and lips, and cheeks, and account for every movement in passion, which is not explained by the direct influence of the mind upon the features.

So we shall find, if we attend to the expression of grief, that the same phenomena are presented; and we may catalogue them, as it were, anatomically. Imagine the overwhelming influence of grief—the object in the mind has absorbed the powers of the frame; the body is no more regarded, the spirits have left it; it reclines, and the limbs gravitate, the whole body is nerveless and relaxed, and the person scarcely breathes; so far there is no difficulty in comprehending the effect in the cause. But why, at intervals, is

there a long drawn sigh, why are the neck and throat convulsed, and whence the quivering and swelling of the lip, why the deadly paleness, and the surface earthy cold; or why does convulsion spread over the frame like a paroxysm of suffocation?

To those I address, it is unnecessary to go farther, than to indicate that the nerves treated of in these papers, are the instruments of expression, from the smile upon the infant's cheek to the last agony of life. It is when the strong man is subdued by this mysterious influence of soul on body, and when the passions may be truly said to tear the breast, that we have the most afflicting picture of human frailty, and the most unequivocal proof, that it is the order of functions which we have been considering that is then affected. In the first struggles of the infant to draw breath, in the man recovering from a state of suffocation, and in the agony of passion, when the breast labours from the influence at the heart, the same system of parts is affected, the same nerves, the same muscles, and the symptoms or characters have a strict resemblance.

Having examined the system of nerves and muscles, which are the agents in respiration, in their fullest extent and in all their bearings; having looked at them, in their highest state of complication in the human body, and having traced them upwards, from the animals of simple structure, and then by experiment, and in a manner analytically as well as synthetically, their relations become obvious. Instead of one respiratory nerve, the *par vagum*, the nerve so called, is found to be the central one of a system of nerves of great extent.

instead of the relations of the vital organs of circulation and respiration depending on some supposed influence of the sympathetic nerve, they are found to have an appropriate system.

This system of nerves, extricated from the seeming confusion in which it lay hitherto encumbered, is found to be superadded to that of mere feeling and agency, attributes common to all animals: through it we see, engrafted as it were, and superadded to the original nature, higher powers of agency, corresponding to our condition of mental superiority: these are not the organs of breathing merely, but of natural and articulate language also, and adapted to the expression of sentiment, in the workings of the countenance and of the breast, that is by signs, as well as by words. So that the breast becomes the organ of the passions, and bears the same relation to the developement of sentiments, as the organs of the senses do to the ideas of sense.

EXPLANATION OF PLATE XXXI.

Fig. 1. Represents the *medulla spinalis*.

A. The *pons Varolii*.

B. B. The anterior medullary columns of the spinal marrow, continued from the *corpora pyramidalia*.

C. *Corpus olivare*.

D. *Corpus restiforme*.

1. The origin of the respiratory nerve of the face.
2. Origin of the glosso-pharyngeal nerve.
3. Origin of the nerve of the par vagum.
4. Origin of the spinal accessory nerve, or superior respiratory nerve of the trunk.

Fig. 2. Plan of the respiratory nerves in their course through the body.

A. *The sterno-cleido-mastoideus muscle.*

B. B. The *trapezius* muscle. It is seen to arise from the back of the head, and from the spine; it is inserted into

C. The scapula, and

D. The clavicle.

E. E. The *serratus magnus anticus.* It is left at its attachment to the ribs, but cut off from its insertion into the scapula, so as to expose the trapezius and the spinal accessory nerve.

F. The lower surface of the diaphragm.

G. The upper surface of the diaphragm.

H. The larynx.

The four great muscles (A. B.B. E.E. F. G.) are associated together, and joined to the organs of the voice, and of smelling, &c. by the nerves displayed here.

To simplify this view, the regular or symmetrical system of nerves is not presented in this drawing, but only the respiratory nerves. It is the entwining of nerves of distinct systems which produces the apparent intricacy. If the spinal nerves were represented crossing these, and the network of the sympathetic superadded to them, we should have all the seeming confusion of the dissected body.

1. Respiratory nerve of the face, or portio dura of authors.

2. The glosso-pharyngeal nerve.

3. The superior respiratory nerve. It is seen to pass through the sterno-cleido-mastoideus muscle, and to supply it with branches: then to take a course

down the side of the neck, branching exclusively to the trapezius muscle.

4. The phrenic or diaphragmatic nerve. It is seen coming out from the spine, and running a direct course to the diaphragm.

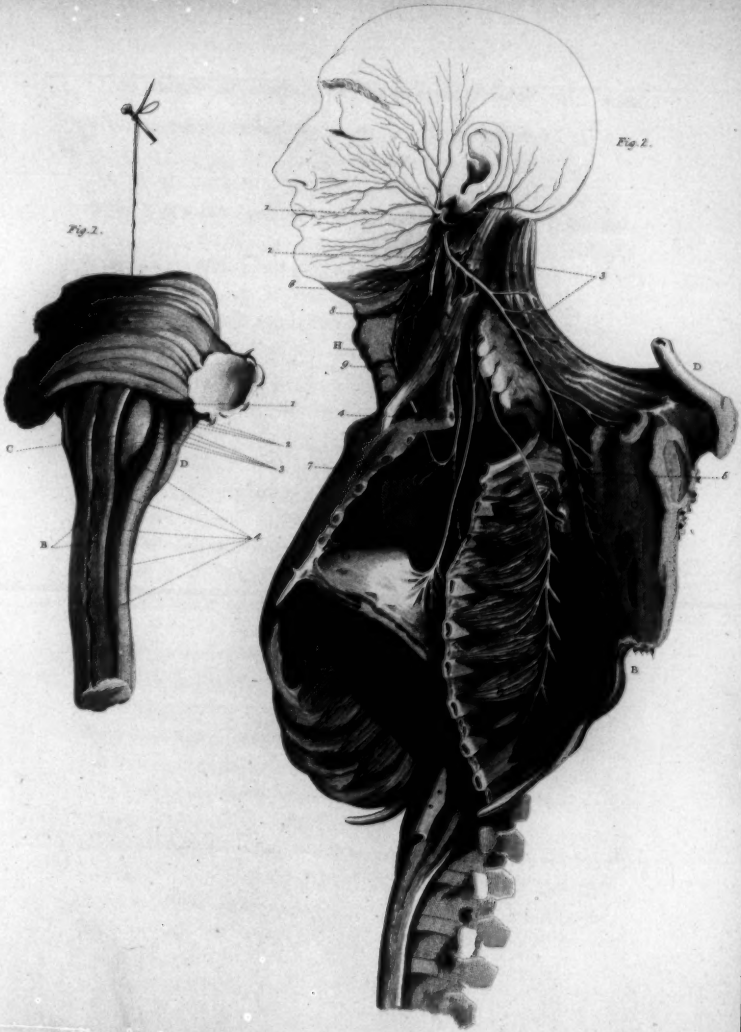
5. The external respiratory nerve of the chest. It is like the last nerve in its origin, but it deviates in its course, passes on the outside of the chest to supply the powerful respiratory muscle, the serratus magnus E. E.

These three nerves combine the mastoid and the trapezius muscles, the serratus magnus and the diaphragm, with the larynx, the tongue, and nostrils.

6. 7. The nerve of the par vagum. Coming from the same origin with the other respiratory nerves, it passes down to the internal organs; but in its passage gives off these:

8. The superior laryngeal nerve, a branch of the last nerve.

9. The recurrent nerve; a branch also of the nerve of the par vagum. Where the nerve of the par vagum is in the thorax (7) at the same time that it sends off the recurrent (9), it sends off many small nerves to the heart and the lungs, and then descends in a plexus on the œsophagus, to the stomach.



Charles Bell. del.t

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XXIV. *Experiments and observations on the Newry pitch-stone, and its products, and on the formation of pumice. By the Right Honourable* GEORGE KNOX, *F. R. S.*

Read May 9, 1822.

THE locality of this mineral, and the singularity of its external characters, having excited my curiosity, I took advantage of the facilities furnished by the liberality of the Royal Society of Dublin, of which I have the honour of being one of the Vice Presidents, to make the subjoined analysis in their laboratory.

Doctor FITTON, in an excellent paper inserted in the first volume of the Transactions of the Geological Society, has given a minute description of the site and external characters of the Newry pitch-stone. I shall transcribe it, previous to laying before the Royal Society my own observations. I do so with more satisfaction, as we in general agree; in fact, scarcely two specimens are exactly the same, although contiguous to each other in the vein; some being compact, some thin slaty, some olive, and some leek-green; some so disintegrated, particularly when exposed to the air, as to be friable between the fingers, while others retain their gloss, consistency, and colour, with much tenacity, although they all fall at length into rhomboidal fragments. I may add also, that while some are quite porphyritic, others have but a few specks of felspar on their surface.

However, Dr. FITTON seems to have overlooked two very striking characters of it, which engaged my particular attention, and gave birth to those conjectures, which principally induced me to undertake the analysis by which they have been verified. I mean the *smell*, and strong oily taste.

"This substance (Dr. FITTON observes), is found in a "vein traversing granite, in the vicinity of Newry, in the "County Down. Its colour is intermediate between moun-"tain and leek-green. It is massive. Fracture small, and "not very perfect conchoidal.

"Internal lustre, resino-vitreous, and shining. It exhibits "lamellar distinct concretions; the plates are from one-"fourth to one-tenth of an inch in thickness, and are further "divisible into pieces of the rhomboidal form of various "angles. The surface of the concretions is smooth and "strongly glistening. Slightly translucent on the edges. It "scratches window glass, but is easily scratched by quartz. "Easily broken. Specific gravity 2,29. Before the blow-"pipe, without addition, it yields a greyish-white frothy "enamel. It is in some places porphyritic, containing im-"bedded minute crystals of felspar and of quartz.

"A letter from a very intelligent observer, who has ex-"amined this substance in its native place, states the follow-"ing particulars respecting its position:

"The vein is first observable in the Townland of Newry, "at the bottom of a bank of granite, about half a mile from "the northern end of the town, on the right of the road lead-"ing to Downpatrick. It crosses the road, and runs due "westward, ending on the side of the great road from Newry

" to Belfast. Its length, so far as hitherto observed, is half " a mile.

" The rock, which is covered with mould to the depth of a " foot, consists of a grey granite. The vein is about two feet " and a half, or two and a quarter in width; at the places " of contact, both the granite and pitch-stone are disinte- " grated, the latter being almost as soft as clay, but becoming " gradually harder as it approaches the center of the vein. " The structure of the vein is foliated, the folia being per- " pendicular to the horizon, and also to the walls; and, be- " sides these, there are seams, that run longitudinally, parallel " to the horizon, and nearly perpendicular to the folia.

" Although this substance presents some peculiarity, in " being divisible into rhomboidal fragments, it approaches in " this respect to the *pitch-stone* of Arran, (in lamellar concre- " tions) which holds as it were a middle place between it, and " that possessing the more usual characters.

" Mr. JAMESON has described a vein of pitch-stone " *running* " in granite" observed by himself in Arran;* and he states " that lamellar distinct concretions have been hitherto ob- " served in the pitch-stone of that island only."†

To the geological account of this mineral, above given, I have only to add, that the granite which it traverses is rather of a soft kind, containing much felspar, and disposed to be converted, by decomposition, into lithomarge; and that a vein of basalt nearly parallel to it, at the distance of about an hundred yards, passes through the same rock of granite. This basalt very readily disintegrates by exposure to air and moisture; and it contains spheroidal concentric balls of basalt of

* Min. of Scottish Isles, 4to. vol. I. p. 81. † JAMESON's Mineralogy, vol. I. p. 361.

a loose contexture; and likewise a considerable quantity of compact stilbite, approaching to fibrous, and of a schistose structure.

I obtained specimens of the pitch-stone of various kinds, compact, thin slaty, and disintegrated; and one of the thin slaty was of a different colour from the rest, darker, more porphyritic, and of a more shining lustre.

The external characters of all agreed in the following particulars.

Specific gravity 2,81. Fracture, small conchoidal; fragments indeterminately angular, rather sharp edged and rhomboidal.—Surface, smooth and glistening.—Feel, unctuous.—Lustre, resinous.—Hardness, semi-hard, the sharp edges scratching glass.—Streak and powder whitish grey, passing into greenish grey.—Smell, *oily*.—Before the blow-pipe, fuses without addition into a pale leek-green glass. Colour of the most porphyritic specimen, perfect leek-green; of the rest, olive-green, passing into oil-green.

Although the peculiar character of this variety of pitch-stone is its smell, yet, I believe, it differs from all others, including those from Arran, in the *degree* in which it is disposed to divide into thin laminæ; its proneness to disintegrate, and the regularity of its rhomboidal fragments. So much is it inclined to disintegrate, that a piece, nearly compact, after lying for some days on the table of a warm laboratory, fell into large rhomboidal fragments.

It will appear, I think, that these qualities proceed from the same cause.

A piece of the compact specimen was exposed for half an hour to a red heat in an open crucible. It lost 7,75 per cent.

of its weight. The colour changed to a pitch brown. It retained its lustre. It opened, in the manner of cannel coal, into thin slaty fragments, but without actually falling in pieces, and the interstices acquired a more greasy lustre, and smoother appearance, than the rest of the stone.

The circumstances above detailed, namely, the *oily smell*, the disintegration, and the effect of ignition, excited a suspicion that the mineral contained some inflammable substance.

One hundred grains of the compact, in fine powder, were exposed to a white heat in a platina crucible, and were converted into a glass of a very pale leek-green colour, and lost ten per cent.

Two hundred and twenty grains of the same, coarsely powdered, were put into a small coated (common bottle glass) retort, to the mouth of which a phial, previously weighed, was luted with white lute, and exposed for half an hour to a red heat. A colourless liquor came over, which had a slightly bituminous smell. The receiver had gained 16 grains, being 7,2727 per cent.

The retort breaking, I was not able to ascertain the loss of weight of its charge.

The stone finely powdered, and projected on melted nitre, scintillated a little.

I now proceeded to my analysis, in the usual way, following the method of KLAPROTH, in his analysis of the pitch-stone from Meissen; my view having been, at first, rather to ascertain ingredients, than proportions.

It may be as well to mention, in this place, the result of KLAPROTH's analysis. It was

Silex	73
Alumina	14,50
Lime	1,00
Oxide of iron	1,00
Oxide of manganese	0,10
Soda	1,75
Water	8,50
	99,85

He obtained his silex by exposing 100 grains of the pulverised stone, along with 200 grains of caustic soda, to a strong red heat, for half an hour, in a silver crucible; when cool, softening with water, dissolving in muriatic acid, evaporating nearly to dryness, diffusing in water, and filtering.

The alumina was obtained by boiling the muriated solution, thus freed from silex, with excess of caustic soda. The alumina was held in solution while the lime and iron were thrown down. The alkali was then neutralised, and the earth precipitated by carbonate of soda. The precipitated lime and iron, first dissolved in muriatic acid, and then combined with sulphuric acid, gave sulphate of lime, which was collected and washed with weak spirits of wine, and the pure lime was estimated as in the proportion of one to three in the sulphate.

The iron was precipitated by carbonate of ammonia. Some flocks, which appeared in the remaining fluid after it had been evaporated to dryness and re-dissolved, were *presumed* to be manganese, and *estimated* at one-tenth of a grain.

I proceeded in the same manner but obtained no manga-

nese; and in the liquor from which the sulphate of lime was extracted, there was no sulphate of magnesia.

The alumina boiled in sulphuric acid and mixed with sulphate of potash was converted into alum, leaving nothing in solution.

The first thing which caught my attention in the course of this process, was the appearance of a black insoluble matter in the muriatic solution from which the silica had been separated. As it evaporated with considerable rapidity, in consequence of the great heat of the sand bath, a rim was left from time to time on the surface of the pan of dry salt, which, from being orange, became white, and shortly after a black powder used to separate from it, fall to the bottom of the pan, and remain undissolved.

This I at first suspected to be oxide of manganese; but having collected it, I found that it was insoluble in acids, and that it burned away at a red heat.

I then conceived that some carbon had accidentally got into the liquor; but as the same phenomena recurred with every experiment, and I repeated my analysis many times, I became convinced that it belonged to the stone. Other circumstances, however, put it out of doubt.

To obtain the alkalies, if any existed in the stone, I pursued each of the methods usually recommended; that invented by Rose, and practised by Klaproth; that of Sir Humphry Davy; the new process by Ceruse; and the action of acids on the stone; by the last of which I finally abided.

Klaproth mixed 100 grains of the pulverised stone with 300 of nitrate of barytes, ignited them in a porcelain crucible, and neutralised the mass with muriatic acid, which he

expelled again with sulphuric acid. The inspissated mass he washed with hot water, filtered, and saturated the filtered liquor with excess of ammonia. The residuum being separated by the filter, the remaining fluid was evaporated to dryness; the sulphate of ammonia having been driven off at a moderate heat, sulphate of soda was left behind. This being dissolved, was decomposed by acetate of barytes, and filtered; the filtered solution evaporated to dryness, and the dry salt exposed to a red heat in a platina crucible. The saline residue was then re-dissolved and evaporated to dryness. It was carbonate of soda.

I have detailed this well known process, chiefly to remark an occurrence in one part of it, which continued to confirm me in the opinion that the stone contained an inflammable substance.

When I was expelling the sulphate of ammonia, I was struck by the melted mass in the crucible, which had before been white, suddenly becoming black, and, as the sulphate of ammonia flew off, gathering into a black powder. This powder being insoluble in water, was easily collected; and, like that which I had got before from the acid solution, resisted the acid, and was volatilised by a red heat.

The alkali which I obtained was soda. By converting it into nitrate, sulphate, and muriate, and observing the crystallization of each, and by testing it with muriate of platina, I ascertained that it contained no potash. It had none of the characters of lithia, neither the crystals of the muriate nor of the nitrate were deliquescent, nor did they tinge the flame when burned with alcohol. When a small portion of the stone, enveloped in soda and placed on platina leaf, was treated by

the blow-pipe, it left no black mark on the metal: it contained therefore no lithia.

I proceed now to my final analysis, premising that, although the last, I think it may be pretty well relied upon. I had made a separate one for each ingredient, giving my whole attention to that which I had selected, without embarrassing myself much with the others.

In this final analysis I adopted a different process, considering it more convenient. My compact pitch-stone being exhausted, I was obliged to resort to the next most compact, and which I call the *slaty compact*.

One hundred grains, mixed with 200 of caustic soda in a silver crucible, were exposed to a red heat. When cool, the contents of the crucible were softened with water and dissolved in muriatic acid. A jelly was formed, by evaporating the solution slowly on a sand bath. When nearly dry, it was diffused in water and filtered. The silica caught on the filter, and exposed to a red heat, weighed 70,8 grains. *(a)*

The filtered liquor *(a)* was evaporated to dryness, and the mass re-dissolved; there remained a powder, which dried at a red heat, weighed 0,75, and was silica. *(b)*

The liquor *(b)* from which the 0,75 of silica had been separated, was precipitated by ammonia and filtered; alumina and iron remained on the filter; the lime went through in solution *(c)*.

The iron and alumina were washed completely off the filter into a silver evaporating dish before they were dry, by projecting against it water, impelled by the breath through a dropping glass; a mode of removing substances from filters always effectual, and far preferable to drying and collecting

the dry powders from the paper. It was suggested by Mr WHARMLEY, Chemical Assistant in the Laboratory of the Royal Society of Dublin.

The alumina and iron thus collected, were boiled in caustic potash until a separation had taken place, the iron remaining as a powder at the bottom of the silver pan, and the alumina being held in solution by the alkali *(d)*.

The iron separated by the filter in the manner above mentioned, was dried and weighed. It weighed 5,25 grains. It was then dissolved in muriatic acid, with which it effervesced, and therefore had been carbonated. A powder remained behind weighing 0,3 grain, which was silica. The iron was then precipitated by caustic potash, filtered, and again evaporated to dryness; it weighed 3,4 grains, which gives of protoxide of iron 3,036 grains.

The solution *(c)* evaporated to dryness and re-dissolved, left a residuum which weighed one grain, and which being insoluble in boiling sulphuric acid, and being perfectly white and gritty, I concluded to be silica. It was then precipitated by carbonate of soda, filtered, washed and dried; the precipitate, which was carbonate of lime, weighed 2 grains. Now, 2 grains of carbonate of lime may be estimated at 1,12 of lime.

The alkaline solution *(d)* was precipitated while hot by muriate of ammonia, filtered, washed, and ignited in a platina crucible; it weighed 11,50 grains.

Extraction of the alkali by acid.

Nitric acid.

100 grains of the pulverised stone were boiled with dilute

nitric acid. When dry, the soluble part was taken up by water and replaced by fresh acid. This process was repeated until the acid seemed to have no farther effect. The aqueous solutions were then thrown together and evaporated to dryness. Alcohol removed the nitrate of lime; and the iron being peroxidated by the nitric acid, nothing remained when the mass was re-dissolved in water but nitrate of soda, which crystallized in cubes, and had every other property of nitre. This being evaporated again to dryness, and weighed, was found to be 7,75 grains of nitrate of soda, which gives 2,857 for soda. I consider this to be the real proportion of the alkali. It differs but little from the following result by

Muriatic acid.

100 grains were treated in the same way with muriatic acid, the iron being peroxidised when the aqueous solution was evaporated to dryness. The chloride of sodium weighed 5 grains. Now, 5 grains of chloride of sodium make of dry soda, or oxide of sodium, 1,98198, being in the proportion 55,5 to 22; but 1,98198 of dry soda produce 2,87044 of hydrate of soda, the state in which it is probable the alkali exists in the stone.

In the process with the muriatic acid two new and decisive phenomena presented themselves.

When evaporating off the excess of muriatic acid from the solution in a platina crucible with a silver cover, I observed the inside of the cover, which I had laid lightly on and removed from time to time, to be coated with a yellow substance, which had a bituminous smell. I placed the cover with the

top down on a chafing dish of charcoal, and in a few minutes the whole was volatilised, and the silver became bright.

On evaporating the alcohol, in order to ascertain how much lime the acid had taken up, I observed a dark oily substance collecting as the alcohol diminished, and which, when the solution was nearly dry, was collected, and weighed 3,70 grains. It had an empyreumatic smell, was insoluble in ether, but dissolved in spirits of turpentine, and inflamed with difficulty with a thick smoke and pungent odour. Naptha dissolved it only in part, and changed the colour to grass-green. It seemed to retain some muriatic acid, and gave indications of containing some iron; it was therefore impure bitumen.

All these circumstances combined to urge me to obtain the bituminous matter of the stone, if possible, in a state of purity, and to ascertain the quantity which existed in it.

I therefore engaged in the following experiments:

I procured an iron retort about 6 inches long and half an inch in diameter, one end of which was screwed to a bent gun-barrel, and the other, or bottom, closed by an iron screw. This apparatus was perfectly well made, and air tight.

Experiment 1.

Into the retort I put 480 grains, in coarse powder, of the dark leek-green thin slaty Newry pitch-stone, as, from its appearance, I judged that that variety contained the greatest proportion of bitumen.

The retort was then enclosed in a long earthen crucible, the interstices being filled up with glass-house sand, and placed in a powerful side furnace. To the end of the gun-

barrel was affixed, and luted with white lute, a glass tube, with a hollow ball near its junction with the mouth of the retort, for receiving liquids, and fitted at the other to a mercurial trough, in order to collect the gases that might come over.*

A considerable quantity of gas came over, in addition to the air of the vessel, and when the retort had acquired a red heat, a liquor distilled which was evidently water.

When the heat was urged farther, another liquor made its appearance. It was slightly coloured and ran down, in oily streaks, into the receiver, where it floated on the water.

When the distillation had ceased the apparatus was removed, all access of air excluded from the receiver, and the gases examined.

They consisted of carbonic acid, which was removed by lime water and caustic potash; of hydrogen, which was judged of by its inflammation; and of carburetted hydrogen, which was tested by adding to a portion of the gas, deprived of carbonic acid, some oxygen gas and exploding by electricity. Carbonic acid was thereby generated, as was proved by its rendering lime water turbid.

The receiver, which had been previously weighed, was now examined. It had gained 7,81 grains per cent.

The oily fluid had the smell of tobacco, exactly that of a tobacco-pipe long in use, and it burned with a similar flame to naptha: while burning it had the same smell as petroleum, which I had collected myself at the Pays de la Poix, near Clermont, in Auvergne. The water was neither acid nor alkaline.

* When I ceased to look for gas, I discontinued the luting.

The experiment was repeated, but without an attempt to catch the gases; and the moment the smell came over the receiver was changed, by which means nearly the whole was obtained, independent of what it suffered from decomposition, and weighed about 2,5 grains per cent.

Experiment 3.

Finding that the iron decomposed the water, and thereby in part the bitumen also, I gave up that apparatus, and had a very strong glass retort, such as is used in the manufacture of common wine bottles, made of a similar shape, and coated, to which a receiver of the same form as those described was luted as before.

I may mention here a very curious appearance of the mass which remained in the iron retort. It was of course of a cylindrical shape; its colour was pale ash-grey; it was porous, semivitrified, and slightly cohering; resembling coarse pumice, and floated on water. But the circumstance to which I now allude is that it separated, when broken, into thin regular tables, precisely in the manner of madreporites, so that it might be considered as schistose pumice.

In this experiment, I gave to 480 grains of the coarsely powdered leek-green variety, a dull red heat for half an hour, in order to expel the water, as I had found that, with that degree of heat, the bitumen was not removed from the stone. I then charged it into the glass retort, inserted in a crucible as before, and gave it a white heat. No gas was generated, nor expelled, and the liquor in the receiver was pure bitumen. It weighed 2,83 grains per cent. and was of a wine-yellow colour, and the smell the same as the last. The neck of the

retort being thinner than the rest had fused, but fortunately not until the whole of the bitumen had come over. The residuum was again a pumice, exactly the same as in the experiments with the iron retort.

Experiment 3.

Slaty compact pitch-stone.

A green glass retort was again charged with the pitch-stone which I had been analysing, namely, the slaty compact; but the neck of the retort, being thinner than the body, fused. On examining the mass within the retort, a light-coloured pumice was found at the bottom, and a dark one at the top.

Experiment 4.

100 grains of the slaty compact pitch-stone which I had been analysing, lost by ignition 8,0 per cent.: when fused it lost 0,5 more, and formed a light ash-grey glass.

Experiment 5.

480 grains of the same were distilled after the water was expelled by ignition, and therefore the danger of decomposition removed, in an iron retort. Bitumen came over, and the process appearing to be completed the receiver was removed, after which some more bitumen dropped from the retort. The bitumen in the receiver smelt more of naptha than the former products, but that which dropped had the same smell of tobacco. What was condensed in the receiver was very volatile, being driven from one end of the phial to the other, namely, by the heat of the hand. The contents of the retort were a light pumice, as before.

Experiment 6.

400 grains of disintegrated Newry pitch-stone which had lost 7,25 per cent. by a red heat, were distilled as usual; 0,1 came over of bitumen, which had the smell and volatility of naptha.

Experiment 7.

100 grains in mass of Meissen *pitch-stone* (the same as that which had been analysed by KLAPROTH, and for which I was indebted to my friend Sir CHARLES GIESECKE, Professor of Mineralogy to the Royal Society of Dublin) were ignited in a platina crucible. The colour changed from oil-green to greenish white passing into greyish green; it opened in the same manner as the compact Newry pitch-stone, showing a tendency to slate, the fissures being parallel; fragments indeterminately angular and rhomboidal, and the lustre altered from resinous to waxy. It was afterwards fused into a white enamel and lost 8 per cent. As KLAPROTH had lost 8,50 by simple ignition, I concluded, erroneously, that it contained no bitumen.

Experiment 8.

400 grains of pitch-stone from Meissen were distilled as usual after ignition: a small quantity of bitumen came over which had the smell of naptha, and was more volatile than any of the former products. The contents of the retort were fused into a greyish white enamel.

Experiment 9.

Having ignited 400 grains of Meissen pitch-stone, and afterwards distilled them in the usual manner, the receiver having

been previously very accurately weighed, 0,5 grain of bitumen were obtained, equally as volatile as that procured in the last experiment. The retort not having received so strong a heat as in the former experiments, only a slightly cohering perfectly white powder remained.

Experiment 10.

On Arran pitch-stone.

I fused 100 grains of pitch-stone from the Island of Arran, in a platina crucible at a white heat; it formed an ash-coloured glass, and lost 5 per cent.

400 grains of the same stone were charged into a green glass retort, as in experiment No. 2. Water came over, and when the heat was increased, was followed by an oily substance; but at the same moment the neck of the retort fused, as in experiment 3, and the distillation ceased. The receiver had acquired an additional weight of 16 grains, that is 4 per cent. It had the smell of the bitumen from the Newry pitch-stone, but faint, and the quantity was too small to be separated. On examining the fused retort, it appeared that, for the distance of an inch from the bottom, the colour of the stone had become, as in former experiments, of an ash-grey, and still resembling pumice, but that the upper part was brown, as in experiment 3; so that it seemed as if the bitumen had risen from the more strongly heated part of the retort, and having been stopped in its progress by the fusing of the neck, had given to the upper part of the mass the same brown colour which I had observed when the whole stone had been originally ignited in an open crucible, as mentioned in the beginning of this paper.

100 grains of the brown part of the residuum of this pitch-stone were then fused, as before, in a platina crucible, into a light-grey glass; it lost one per cent.; thus making up the 8 per cent. which had been the loss from the original fusion.

Experiment 11.

400 grains of Arran pitch-stone lost by ignition 4.5 per cent.; and when distilled in the usual manner in an iron retort, one grain was collected in the receiver, possessing a very peculiar smell. The contents of the retort were a perfect enamel. The receiver was washed with sulphuric ether, which was suffered to evaporate, and a substance remained on the glass pan, but too small in quantity to enable me to examine it.

Experiment 12.

Artificial pumice.

100 grains of artificial pumice, which had been the result of my first and second experiment, were fused in a platina crucible into a greyish-white glass. It lost nothing.

OBSERVATIONS.

I shall, for the sake of distinctness, divide my remarks under heads, beginning with the analysis.

Analysis.

Having mistaken the bitumen which appeared so frequently in the course of all my analyses of the stone, for manganese, and having found, I think, that inflammable substance* in the

* Vide Experiment.

Meissen pitch-stone, will it be presumption to suspect that KLAPROTH made a similar mistake; and that the substance, which he does not appear to have examined, and which he acknowledges he did not weigh, was, in reality, bitumen? This cannot affect the character of this admirable analyst, since there was nothing in the appearance or external characters of the Saxon mineral which could awaken a doubt on that subject; whereas, the peculiar properties of the Newry pitch-stone, naturally, and early, arrested my attention, and would equally have engaged his, had it been submitted to him.

KLAPROTH's assertion, however, that his pitch-stone contained manganese, induced me to institute many experiments to obtain the same substance from mine, and in the course of them I had occasion to try the different methods of separating manganese from iron, recommended by HATCHETT and HERSCHEL.

I mixed perfectly pure oxides of each metal in ascertained proportions, and dissolved them together in muriatic acid, and I found they could be perfectly separated by both methods. I had, at first, some doubts as to HERSCHEL's, but found that my failure had arisen from neglecting to stir the liquor during the precipitation of the iron. In general, I think, the separation is more easy by HATCHETT's mode, but, under some circumstances, HERSCHEL's will be found preferable.

There is one method of obtaining, in an analysis of a stone, the iron combined with the silica alone, and as muriatic acid easily separates them, it may sometimes be found useful. The heat which decomposes muriate of iron leaving an insoluble oxide, does not decompose the earthy muriates; therefore,

if the mass be dried on the sand bath at a heat below redness, when it is afterwards diffused in water, the salts will be dissolved, and the iron and silica will be obtained on the filter.

With respect to the alkalies, the fusion with nitrate of barytes is a tedious process, and liable to loss from the frequent filtrations, and also from the necessity of evaporating off the sulphate of ammonia.

That of Sir HUMPHRY DAVY, by boracic acid, is much neater; but, probably from want of analytical skill, I found it so difficult to get rid of the boracic acid, that I was not able to rely upon my result.

The new method, by lead, has its embarrassments also, particularly from the fusibility of the glass which acts upon platina, and penetrates clay crucibles. There is also a good deal of difficulty in getting entirely rid of the lead.

The process by acids, where the nature of the stone will admit of it, is obviously the best; and I found that the nitric acid was to be preferred, as, by its peroxidizing the iron, and destroying the bitumen, those two troublesome substances were disposed of.

Bitumen.

It has appeared, from the foregoing analysis, that this substance may be obtained both in the usual mode of analysing, and by distillation of the stone, but quite pure only in the latter mode. It seems to consist of two inflammable substances, the one much more volatile than the other, but both inseparable from the stone, except at a heat approaching, if not entirely amounting to, whiteness. I imagine that it is in combination with the iron, as it seemed in general to accom-

pany the solutions of that substance, and to modify the colour and magnetic properties of the metal.

It is necessary to observe that, unless the beak of the retort be kept almost at a red heat, as far as the mouth of the receiver, the whole of the bitumen will not be obtained.

This quality renders it easy, however, to ascertain the proportion in which it exists in the stone; a heat of ignition expelling only the water, and the loss of weight by fusing after ignition, giving pretty exactly the quantity of this curious inflammable matter, which appears to vary considerably in the different specimens from the same vein.

I have not yet been able to examine it with sufficient accuracy, partly from want of time, and partly from the difficulty of analysing so small a portion of the substance as I have yet been able to collect. I hope, however, to renew the investigation next winter. If it should be found to be a new substance, I propose to call it *newrine*. I should not be surprised, however, judging from the smell, and its being separable from water by evaporation, if it were found to contain *nicotine* in combination with naptha.

Pumice.

The formation of this substance *artificially*, is rather an interesting circumstance, if it be new; and may throw some light upon its natural formation.

That it is a perfect pumice, in appearance and qualities, what I have already mentioned puts almost out of doubt. It has the colour, levity, and magnetic properties of the real pumice, and deceived artists to whom I presented it as such. An eminent paper stainer, who is in the constant use of that

article, and to whom I sent two pieces, desiring to know, from him, which of the two specimens was the best; answered, that both were excellent, but that the lighter was the best. He had no suspicion that they were an artificial product. If the analysis of pitch-stone and pumice be considered together, it will be found that the water, bitumen, and lime, in the former, are the substances which alter the per centage.

How the lime was got rid of by the volcanic heat is not easily accounted for; but it is conceivable that it may have been acted on by the muriatic acid fumes, which, we know, accompany volcanic fires. Besides, lime may yet be found in some pumice, and none of that earth in some pitch-stones. As yet, I believe, there have been but two pitch-stones, the Meissen and the Newry, analysed, and the former only by a chemist of authority. It must be acknowledged, however, that it is difficult to imagine, on this hypothesis, how the alkali remained un-muriated, while the lime united to the muriatic acid.

It appears to be a condition, in converting a stone into pumice, that it should contain a volatile substance, which can only be removed by the same degree of heat which is at the same time necessary for producing that sort of semi-vitrification in the mass which renders it coherent, hard, and porous. If a stone contains only water, pumice is not formed, because that is driven off by a red heat, which does not act upon the earths of which the stone is composed. Drive off the water at a red heat, and you have a harder but incoherent mass: increase the heat, and you have, as in the case of alumine, a more compact and denser substance, or else, when the ingredients of the stone favour vitrification, a glass.

I have made pumice in an open crucible, and with the unpowdered stone, by watching the degree of heat.

Magnetic properties.

A needle, drawn a little out of the direction of the poles by a piece of iron, was drawn back by all the pitch-stones, and by pumice, natural and artificial.

The pumices, as well the natural as the artificial, and the latter, whether obtained by distillation in glass or iron, were attracted, when in powder, by the magnet; but the magnet did not act on any of the pitch-stones under any circumstances.

RESULT.

If the foregoing analysis is correct, that variety of the Newry pitch-stone which I examined contains in one hundred parts,

Silica	72,800
Alumine . . .	11,500
Lime . . .	1, 12
Protoxide of iron .	3,086
Soda	2,857
Water and bitumen	8,500
	99,813

The *Newry* pitch-stone is a *marked variety* of that substance.

It exists in various forms, and the bitumen in various proportions in the same vein.

It contains, as far as at present appears, a peculiar bitumen.

That bitumen exists in the Newry pitch-stone in a larger proportion than in any substance of that class hitherto examined.

The darker specimens appear to contain most bitumen, nearly 3 per cent.

The bitumen, as well as the soda, can be separated from the stone by the action of nitric and muriatic acid, but the bitumen not in a state of purity.

The bitumen can be obtained pure by distillation from the stone by means of a heat nearly approaching to whiteness.

It can be separated from water by evaporation. The Newry pitch-stone is converted into pumice when properly distilled.

It is magnetic, but not so much so as its pumice.

It is not electric, either by heat or friction, neither is its pumice.

It probably owes its *smell*, *slatiness*, and disposition to *disintegrate*, to the bitumen.

The bitumen probably modifies its *colour* also.

The Newry stone agrees, as to its ingredients, with the Saxon pitch-stone from Meissen; but they differ somewhat in their relative proportions, and particularly in the quantity of bituminous matter.

The Arran pitch-stone seems also to contain bitumen; but on this point I cannot yet form a decided opinion.

I had begun a course of experiments on other substances, namely, green-stone, basalt, obsidian, &c., but I have been obliged to discontinue them for the present. I cannot however abstain from mentioning one experiment which failed, as it was attended by a circumstance which gives it some interest.

I ignited 400 grains of green-stone, which lost 5 grains; and charged it into an iron retort as usual. The receiver increased 0,2 in weight, and there was no smell. On examining the contents of the retort after the distillation, I found that the powdered stone had been converted at the bottom into a vesicular glass, and at the top into a pumice, but that a hole had opened in the upper part of the neck of the retort. It seemed therefore that, in the first place, most of the volatile matter must have escaped, whatever that volatile substance may have been, through the hole, but that the temperature having been lowered in its vicinity by the admission of air, the formation of glass was checked as far as that influence extended, and the result was pumice. Why the glass was vesicular and not compact, I shall leave to conjecture. It is probable that farther experiments may elucidate that point.

NOTE.

To assist in forming a judgment whether manganese really existed or not in the Meissen pitch-stone, to 100 grains of the pulverised stone was added 0,1 of oxide of manganese, and fused. The colour was whitish, as before, but as the great heat necessary to fuse the substance might have discoloured the manganese, the following experiments were made. To 99,9 grains of pure white glass, 0,1 of black oxide of manganese was added. It fused into a violet-coloured glass; but when the heat was increased the colour was removed. To try, therefore, whether a violet glass might be produced from a fusion at a lower temperature of

the Meissen pitch-stone, it was first fused with borax by the blow-pipe: no trace of a violet-colour appeared, but, when afterwards fused by the blow-pipe with 0,1 addition of black oxide of manganese, the colour produced was a violet.

XXV. *Observations on the changes the egg undergoes during incubation in the common fowl, illustrated by microscopical drawings.* By Sir EVERARD HOME, *Bart. V. P. R. S.*

Read May 16, 1822.

THIS has been a favourite subject with many of the most celebrated anatomists. The great HARVEY, MALPIGHI, and JOHN HUNTER, have done more than any others in the advancement of this enquiry.

HUNTER's observations were never rendered so complete as to induce him to publish them; but what he has done is open to the public, both in the preparations in his Collection, and in his Cabinet of Drawings.

In these last, the vesicle which begins near the rectum, and afterwards envelopes the embryo, is clearly made out.

In the year 1815, Mr. DUTROCHET, an ingenious French anatomist, laid a Memoire upon this subject before the French Academie. Mr. G. CUVIER has given a Report upon this Memoire, but has not given the Memoire itself. He considers the author to be the first person who has clearly explained the rise and progress of the vesicle; but some of Mr. HUNTER's drawings of it are dated 1778, the year I went to live in his house.

After all that has been done, much is still wanting to render the investigation complete; and to promote this object, the following Observations and Drawings are brought before the Society.

The gelatinous molecule, from which the future embryo

is to be formed, is originally placed on the surface of the yelk; it is found there before the yelk leaves the ovarium, and lies loose upon it, not being enveloped in any capsule.

The external membrane of the yelk is very thin and delicate, its surface is studded over with red dots, which disappear in its passage along the oviduct. When this is removed, there is a second thick and spongy covering under it, in which there is a natural aperture; and the areola, surrounding the molecule, is nothing more than the surface of the yelk, that is circumscribed by the margin of this aperture. No such aperture has been before taken notice of.

The molecule itself has a granulated appearance; in the centre it is made up of globules $\frac{1}{3800}$ part of an inch in diameter, surrounded by circles of a mixed substance, consisting of about two-thirds of the same small globules, and one-third of larger oval globules, about $\frac{1}{1600}$ part of an inch in length, and $\frac{1}{3000}$ part in diameter; these last, in their figure, resemble the red globules of the blood in the bird in every respect, excepting their red colour. Besides the globules there is some fine oil, which appears in drops when the parts are immersed in water. Oval globules and oil are also met with in the yelk itself, but in small proportion, and without colour.

All these parts, except the red dots on the surface, are met with in the yelk on a smaller scale, even six days before it is completely formed.

The ovarial yelk-bag gives way at the middle line, farthest from the insertion of the blood-vessels, and the yelk drops out into the mouth of the oviduct.

The yelk-bag does not immediately close, although it con-

tracts considerably: some time after it is nearly obliterated; and on the pedicle, the rudiments of a new yelk are formed.

The yelk-bags are exceedingly vascular; the outer membrane of the yelks is connected to them by vessels and fasciculi of fibres, but easily separated.

The yelk, while in the ovarium, has an oval form, and lies with its long axis towards the pedicle of the bag. See Plate XXXII. figures 1, 2, 3, 4, 5.

In its passage along the oviduct the yelk acquires the albumen, and before it comes to the lower end, the albumen is covered by a very fine membrane.

In this passage the thread-like substances, called by Mr. HUNTER the poles, by others the chalazes, are formed; and terminate in the double membrane, which is added at the time the egg has reached the enlargement at the lower end of the oviduct. In the cloacus the shell is formed. If the egg is taken out before it has acquired a shell, it remains soft for several days; in one instance, after four days, it was so semi-transparent as to receive a yellow tint from the yelk. On puncturing the covering, the contents rushed out, but immediately resumed their form, being inclosed in the thin membrane of the albumen. The molecule with its areola, and the chalazes, were distinctly seen. The whole contents had less volume than the shell, particularly in the long axis, being truncate at both extremities. When immersed for an hour and half in distilled vinegar, the albumen and other parts had become somewhat coagulated. Plate XXXII. fig. 6, 7, 8, 9.

In the new laid egg the appearances are exactly the same,

whether it is impregnated or not. Plate XXXII. fig. 10, 11, 12.

When the shell and membranes under it are removed from one side, the yelk appears to be kept in its place by the poles, although allowed to rotate upon its axis.

The gelatinous molecule, with its areola, is always found upon the highest point of the upper surface of the yelk. Whether this arises from the molecule, or from the areola being the lightest part of the yelk, has not been ascertained.

When the hen begins to sit, a new laid egg, which has not been allowed to cool, will have the rudiments of the embryo formed some hours sooner than in the other eggs.

Having traced the formation of the egg itself through all the changes that take place from the time the yelk leaves the ovarium till it is impregnated, we are now to follow those changes that are met with during incubation till the embryo becomes a completely formed chicken.

In 4 hours after incubation the outer edge of the areola had become enlarged, and that part of it next the molecule appeared darker. One end of the molecule appeared like a white line, the first rudiments of the embryo. Plate XXXIII. fig. 1, 2, 3.

In 8 hours, the white line was found to be extended, and the rudiments of a brain and spinal marrow were formed, surrounded by a membrane, which afterwards becomes amnion.

The areola had extended itself, and the surface beyond the line which formed its boundary had acquired the consistence of a membrane, and had also a distinct line by which it was

circumscribed. This I shall call the outer areola. In the space between these two areolas, there were distinct dots of an oily matter. This extension of membrane to the outer areola lies under the inner membrane of the yelk, and can readily be removed entire. Plate XXXIII. fig. 4, 5, 6.

In 12 *hours*, the rudiments of the brain were more distinct, as well as of the spinal marrow. These parts were placed upon a black ground in vinegar, and hardened; the upper end showed the tuberculum annulare of the brain, from which passed down two semi-transparent lines resembling an appearance peculiar to the spinal marrow of the bird. Plate XXXIII. fig. 7, 8, 9.

In 16 *hours*, there was a farther advance in the structure of all these parts. Plate XXXIV. fig. 1, 2, 3.

In 24 *hours*, a still greater increase. Plate XXXIV. fig. 4, 5, 6.

In 36 *hours*, the head was turned to the left side. The cerebrum and cerebellum appeared to be distinct bodies; the iris was seen through the pupil of the eye. The intervertebral nerves were nearly completely formed; those nearest the head the most distinct. A portion of the heart was seen.

At this period, under the inner areola, apparently at the termination of the spinal marrow, a vesicle had begun to protrude. In some eggs it is seen earlier than in others, and has been observed before the heart had become visible. Plate XXXIV. fig. 7, 8, 9.

In 2 *days* 12 *hours*, the spinal marrow was found to have its posterior part enclosed; the auricles and ventricles of the heart were seen, the auricles filled with red blood. An arterial trunk from the left ventricle gave off two large

vessels, one to the right side of the abdomen of the embryo, the other to the left, sending branches over the whole of the areolar membrane, which was bounded on each side by a large trunk carrying red blood; but the two trunks did not unite, there being a small space on one side rendering the circle incomplete.

The vesicle was somewhat increased in size; it lay in the lower part of the abdomen, the parietes of which were not yet formed. Plate XXXV. fig. 1, 2, 3.

In 3 days, the outer areola had extended itself over $\frac{1}{3}$ of the circumference of the yelk, carrying the marginal arteries along with it to the outer edge, but diminished in size. The brain was much enlarged, consisting of four cavities containing a fluid, the cerebellum still the largest. The spinal marrow and its nerves were more perfectly formed. The eye appeared only to want the nigrum pigmentum.

The right ventricle of the heart contained red blood: the arteries could be traced to the head: the rudiments of the wings and legs were formed: the vesicle was farther enlarged, but its vessels did not carry red blood. It had forced its way out through the external covering of the yelk, and opened a communication through this slit, by which a part of the albumen was admitted to mix itself with the yelk, and give it a more oval form. At this period the embryo is generally found to have changed its position, and to be wholly turned on the left side. Plate XXXV. fig. 4, 5, 6.

In 4 days, the vesicle was more enlarged and more vascular, its vessels containing red blood.

The optic nerve and nigrum pigmentum of the eye were visible; the other parts had become more perfectly formed.

The outer areola had extended itself half over the yelk, which had now become still more encreased in size, a greater portion of albumen having become mixed with it. Plate XXXV. fig. 7, 8, 9.

In 5 days, the membranous bag that formed the vesicle had acquired a great size, and become exceedingly vascular in its coats; the cavity contained a fluid. The yelk had become thinner in its consistence, more of the albumen having been mixed with it. Plate XXXVI. fig. 1, 2, 3.

In 6 days, the vascular membrane of the areola had extended farther over the yelk. The vesicle at this time had suddenly expanded itself in form of a double night-cap over the yelk and its coverings, beginning to enclose the embryo.

This change is so rapid, that it has been with difficulty detected, and different accounts have been given of the mode in which it takes place.

The amnion contained water, in which the embryo floated, suspended by the vessels supplying the areolar and vesicular membrane.

The brain had become enlarged to the size of the body of the embryo: its vessels were distinctly seen. The two eyes equalled in size the whole brain: the marsupium was seen covered with nigrum pigmentum. The vessels of the cerebellum could be traced into the convolutions of the pia mater.

The parietes of the thorax and abdomen had begun to form; the wings and legs were nearly completely formed, as well as the bill. At this period muscular action was first noticed. Plate XXXVI. fig. 4, 5, 6.

In 7 days, the vesicle having extended over the embryo,

had begun to enclose the areolar covering of the yelk, and a pulsation was distinctly seen in the trunk that supplied the vesicular bag with blood. The pulsations were 79 in a minute while the embryo was kept in the temperature of 105°, but when the temperature was diminished the pulsation ceased, and when restored was reproduced. The pulsation was kept up, by attention being paid to the temperature, for 36 hours. Muscular action had become vigorous in the limbs.

When the embryo was completely immersed, although the temperature of the water was 108°, the pulsations immediately ceased, the blood being no longer aerated. Plate XXXVI. fig. 7, 8, 9.

In 8 *days*, the anastomosing branches of the vesicular circulation had the arterial pulsation very strong in them. Plate XXXVII. fig. 1, 2, 3.

In 9 *days*, the vesicle had nearly enclosed the yelk, but not intirely; for when the embryo was turned upon its back, and the opposite surface examined, a portion of yelk was unenclosed, and beyond it some of the albumen was met with not mixed with the yelk. Plate XXXVII. fig. 4, 5, 6.

In 10 *days*, the vesicle was opened, and the upper half turned aside. When the amnion, which had become full of water, was opened and the embryo taken out, the thorax was found completely enclosed; the roots of the feathers were distinct, and the passage for the areolar as well as the vesicular vessels exposed.

In 14 *days*, the yelk remained out of the body. When the thorax and abdomen were opened, and the heart as well as the lobes of the liver turned aside, the trunks of the blood-vessels could be traced to the heart; but as the arteries

immediately after death become empty and the veins remain full, the vein from the vesicle, and that from the areola were the most conspicuous.

In 18 *days*, the greater part of the yelk was drawn into the body.

In 20 *days*, the chick was completely formed; the yelk was entirely drawn in, and only some portions of the membrane of the vesicle appeared externally. The yelk passed into the intestine a little way above the openings of the cœca.

Having traced the progress of the formation of the chick step by step, from the first appearance of the molecule found on the yelk before it leaves the ovarium to the complete evolution of all its parts, and its leaving the shell, I shall now take advantage of this investigation, which, illustrated as it is by Mr. BAUER's microscopical drawings, will stand long without a parallel.

Although the processes by which the human fœtus and that of quadrupeds are formed, differ in many essential particulars from those employed in the bird, some circumstances are common to both; one of these, the mode in which the vesicle bursts the membranes of the yelk is distinctly seen in the embryo in the egg hatched out of the body, and explains what takes place where the embryo is attached to the uterus, which otherwise could not have been ascertained.

In a former paper, I have shown the formation of the ovum in the corpus luteum; I have traced this ovum into the uterus, where it became concealed in the soft flocculent bed of efflorescent coagulable lymph prepared to receive it. That it is afterwards enveloped in this moss-like covering, has been demonstrated by my revered master in Human Anatomy,

Dr. WILLIAM HUNTER; but in what manner the embryo breaks its amnion, and opens a communication with the uterus, is a question on which Dr. HUNTER is silent: indeed it is one that could not be touched upon till it was ascertained that the ovum, when it arrived at the uterus, was completely enclosed in the amnion.

Having been the first to ascertain that curious fact, and having arrived at that knowledge through the microscopical observations of Mr. BAUER, I am peculiarly gratified, that the same powers, exerted in another correspondent investigation, should have enabled me to give the solution.

In the ovum of the hen, the rudiments of the embryo require nourishment from one source, aeration from another. In the human ovum one source supplies both; but in either case there is a necessity for a communication between the blood vessels of the embryo and the source of their blood's aeration; and this is effected in exactly the same manner in the human species, the quadruped, and the bird. Out of this the embryo acquires a mode of suspension in the amnion which secures it from injury, both in the bird and quadruped. The mode is as follows: that bag, which is afterwards in the human species and in quadrupeds to become the urinary bladder, is enlarged to a certain size with such rapidity, that it bursts the amnion, which is prepared to be so ruptured, and the arteries lying upon its two sides are carried directly into contact with the chorion, and there the placenta is formed in the open space between the two edges of the chorion. This is exactly similar to the vesicle in the chick bursting the covering of the yelk, and forming the enveloping bag. It is deserving of remark, that in the human ovum, and that of

quadrupeds, there is a natural opening in the external membrane; in the bird, the natural opening is in the internal one, which in its structure bears a resemblance to the chorion.

EXPLANATION OF THE PLATES.

Plate XXXII.

Fig. 1. The ovarium of a hen that had been laying for some time. Natural size.

Fig. 2. A yelk taken out of its bag, and lying in its natural position horizontally. Natural size.

Fig. 3. A small portion of the yelk, with the molecule lying on the bare surface of the yelk within the aperture of the inner membrane of the yelk; the external membrane is turned aside, and shows a concave aperture which circumscribes exactly the molecule. Magnified 5 diameters.

Fig. 4. The bare molecule after being hardened in distilled vinegar, laid on a black ground. Magnified 10 diameters.

Fig. 5. The globules of which the inner areola is chiefly composed. Magnified 400 diameters.

Fig. 6. An egg found in the oviduct of the same hen from which the above ovarium was taken. Natural size.

Fig. 7. The same egg, its soft and semi-transparent shell being removed, the albumen and yelk are enclosed in the double membrane which lines the shell. Natural size.

Fig. 8. A small portion of the yelk of the same egg, with the molecule lying on its bare surface within the orifice of the inner membrane, and the external membrane turned aside. Magnified 5 diameters.

Fig. 9. The same molecule hardened, and laid on a black ground. Magnified 10 diameters.

Fig. 10. A new laid egg. Natural size.

Fig. 11. A small portion of the yelk of the same egg, with the molecule lying on its bare surface, and the external membrane of the yelk turned aside. Magnified 5 diameters.

Fig. 12. The molecule hardened in vinegar and laid on a black ground. Magnified 10 diameters.

PLATE XXXIII.

Fig. 1. *An egg, opened 4 hours* after incubation. Natural size.

Fig. 2. A small portion of the yelk, with the molecule and areola under the external membrane of the yelk. Magnified 5 diameters.

Fig. 3. The bare molecule, hardened and laid upon a black ground. Magnified 10 diameters.

Fig. 4. *An egg, opened* 8 *hours* after incubation. Natural size.

Fig. 5. A small portion of the yelk, with the molecule and enlarged areola lying on its surface under the external membrane. Magnified 5 diameters.

Fig. 6. The same molecule with its enlarged areola, hardened, and laid on a black ground. Magnified 10 diameters.

Fig. 7. *An egg, opened 12 hours* after incubation. Natural size.

Fig. 8. A small portion of the yelk, with the embryo and areola lying on its surface under the external membrane. Magnified 5 diameters.

Fig. 9. The same embryo and areola hardened and laid on a black ground. Magnified 10 diameters.

Plate XXXIV.

Fig. 1. *An egg, opened* 16 *hours* after incubation. Natural size.

Fig. 2. A small portion of the yelk, with the embryo and areola lying on its bare surface, and the external membrane of the yelk entirely removed. Magnified 5 diameters.

Fig. 3. The same embryo, and a portion of the areola laid on a black ground. Magnified 10 diameters.

Fig. 4. *An egg, opened* 24 *hours* after incubation. Natural size.

Fig. 5. A small portion of the yelk, with the embryo and areola lying on its surface, and the external membrane of the yelk entirely removed. Magnified 5 diameters.

Fig. 6. The same embryo, and a portion of the areola laid on a black ground. Magnified 10 diameters.

Fig. 7. *An egg, opened* 36 *hours* after incubation. Natural size.

Fig. 8. A small portion of the yelk, with the embryo and areola lying on its surface, and the external membrane of the yelk removed. Magnified 5 diameters.

Fig. 9. The same embryo, and a portion of the areola laid on a black ground. Magnified 10 diameters.

Plate XXXV.

Fig. 1. *An egg, opened two days and twelve hours* after incubation. Natural size.

Fig. 2. A portion of the yelk, with the embryo lying in its natural position in the centre of the vascular areola, and the external membrane of the yelk removed. Magnified 6 diameters.

Fig. 3. The same embryo, with the principal branches of its blood-vessels, turned upon its back, and laid upon a black ground. Magnified 6 diameters.

Fig. 4. *An egg, opened 3 days* after incubation. Natural size.

Fig. 5. A portion of the yelk and vascular areola, with the embryo in its centre, lying entirely on its left side, which is its natural position. Magnified 6 diameters.

Fig. 6. The same embryo turned on its right side, and laid on a black ground. Magnified 6 diameters.

Fig. 7. *An egg, opened 4 days* after incubation. Natural size.

Fig. 8. The embryo in its amnion, and with its vesicle removed from the yelk, and laid in its proper position on a black ground. Magnified 4 diameters.

Fig. 9. The same embryo without its amnion, turned on its back, and laid on a black ground. Magnified 4 diameters.

PLATE XXXVI.

Fig. 1. *An egg, opened 5 days* after incubation. Natural size.

Fig. 2. The embryo in its amnion, and with its vesicle removed from the yelk, and represented in its natural position. Magnified 3 diameters.

Fig. 3. The same embryo taken out of its amnion, and turned upon its back, having on its right side the increasing vesicle, and on its left a small portion of the areolar membrane with the principal trunk of the blood-vessels. Magnified 3 diameters

Fig. 4. *An egg, opened six days* after incubation. Natural size.

Fig. 5. The embryo in its amnion, and with its vesicle removed from the yelk, and represented in its natural position. The vesicle at this period forms an adhesion with the amnion at the lower extremity. Magnified 3 diameters.

Fig. 6. The same embryo turned on its back, having on its right side a small portion of the vesicular, and on its left, of the areolar membrane, with the principal trunks of their blood-vessels. Magnified 3 diameters.

Fig. 7. *An egg, opened the seventh day* after incubation. Natural size.

Fig. 8. The embryo in its amnion, and with its vesicle removed from the yelk, and represented in its natural position. The vesicle, which at this period covers entirely the embryo, is here turned downwards, and shows the increasing adhesion with the amnion. Magnified 2 diameters.

Fig. 9. The same embryo extracted from its amnion, and turned upon its back, having on its right side a small portion of the vesicular, and on its left, of the areolar membrane, with the principal trunks of their blood-vessels. Magnified 2 diameters.

PLATE XXXVII.

Fig. 1. *An egg, opened* 8 *days* after incubation. Natural size.

Fig. 2 The embryo within its amnion, in its natural position, and with a portion of the vesicular and areolar membranes, with the principal trunks of their blood-vessels. Magnified 2 diameters.

Fig. 3. The same embryo turned on its back, having small portions of the vesicular and areolar membranes protruding from the enclosed part of the abdomen. Magnified 2 diameters.

Fig. 4. *An egg, opened nine days* after incubation. Natural size.

Fig. 5. The same egg turned more to its right side, to show that at this period, neither the vesicular, nor the areolar membranes, yet enclose the whole yelk, nor is the albumen entirely absorbed. Natural size.

Fig. 6. The embryo of the same egg, with a portion of the vesicular and areolar membranes, and their principal blood-vessels; the amnion is opened as far as the vesicular adhesion admits. Magnified 2 diameters.

Fig. 7. *An egg, opened ten days* after incubation. Natural size.

Fig. 8. The same egg with the external half of the vesicle removed, showing the embryo distinctly in its amnion, and the inner half of the vesicle, with its blood-vessels covering the whole. Natural size.

Fig. 9. The embryo of the same egg, with the amnion and the vesicle entirely removed, to show the opening in the abdomen, from which portions of the vesicular and areolar membranes and turns of the intestines are protruding; the roots of the feathers are now visible. Magnified 2 diameters.

PLATE XXXVIII.

Fig. 1. *An egg, opened fourteen days* after incubation. Natural size.

Fig. 2. The same egg, with the external half of the vesicle removed, to show the embryo in its amnion, and that the yelk is not yet entirely enclosed by the areolar membrane. Natural size.

Fig. 3. The embryo of the same egg; its thorax and ab-

domen opened; the heart and the lobes of the liver turned aside to show the course of the principal trunks of the blood-vessels which go to the vesicle and to the areolar membrane; the yelk, at this period, is still entirely without the body of the embryo. Magnified 2 diameters.

A.A. a portion of the amnion. B.B.B. the vesicle. C.C. a portion of the areolar membrane.

Plate XXXIX.

Fig. 1. *An egg, opened eighteen days* after incubation. Natural size.

Fig. 2. The same egg, with the external half of the vesicle removed. Natural size.

Fig. 3. The chick taken from the same egg; the thorax and abdomen laid open to show that the greater part of the yelk is now drawn into the body. Natural size.

Plate XL.

Fig. 1. *An egg, opened twenty days* after incubation, the vesicle and amnion entirely removed, to show the natural position of the perfectly formed chick. Natural size.

Fig. 2. The chick taken out of the same egg, to show that the yelk is now entirely drawn into its body, only some small portion of the vesicular membrane protruding from the orifice of the abdomen, which is not yet quite closed. Natural size.

Fig. 3. The same chick opened, to show the natural position of the viscera and the yelk at this period of the incubation. Natural size.

PLATE XLI.

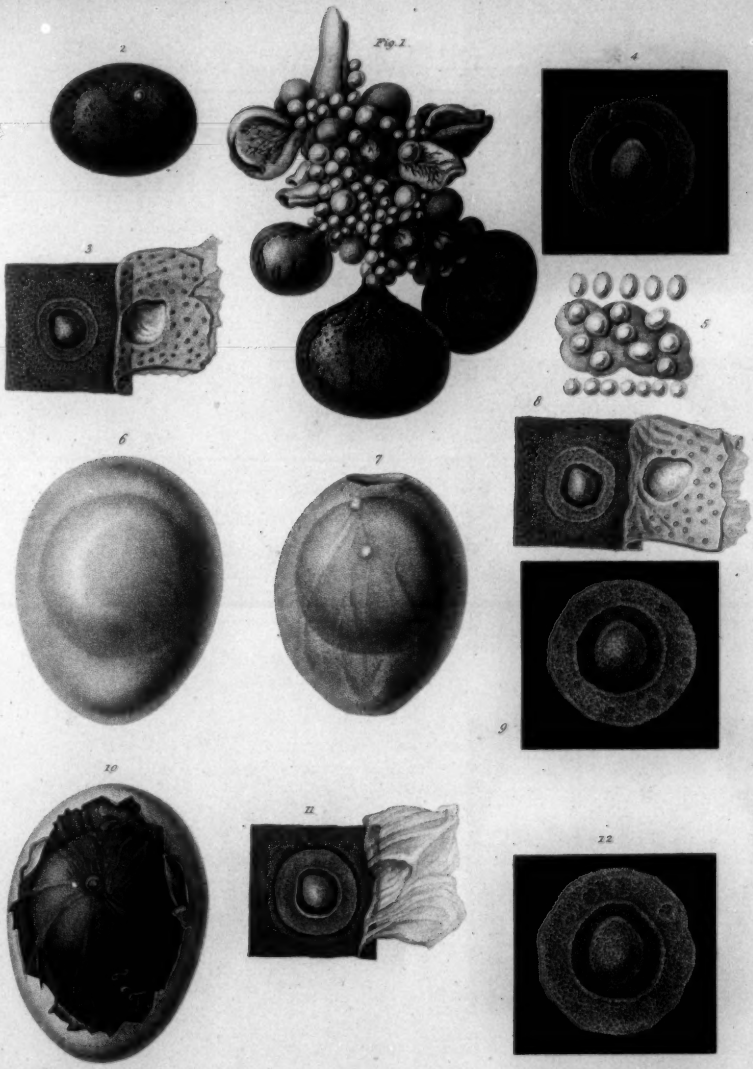
Fig. 1. *A chick, after being hatched 24 hours;* the thorax and abdomen opened, to show the natural position of the viscera and yelk; the crop at that time is full of corn, and other food. Natural size.

Fig. 2. The viscera of the same chick; the heart turned upwards, and the two lobes of the liver laid aside, to show that the vena cava is a continuation of the vesicular vein, and terminates in the right auricle of the heart, after receiving the venæ hepaticæ. Magnified 2 diameters.

The areolar vein terminates in the vena portarum, which is not seen in this view.

Fig. 3. A posterior view of the gizzard and œsophagus, to show the origin of the duodenum, and the course of the intestinal canal, of the same chick. Magnified 2 diameters.

Fig. 4. An anterior view of the rectum, and colon, to show at A, where the great mass of the yelk has been cut off. Magnified 2 diameters.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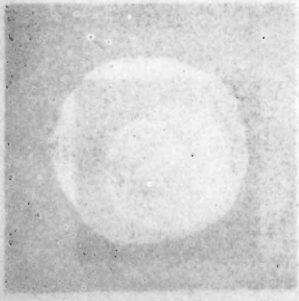
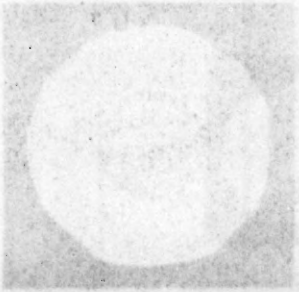

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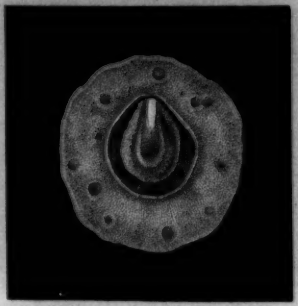

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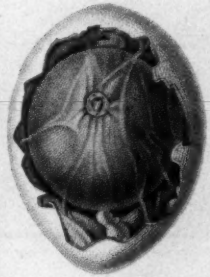

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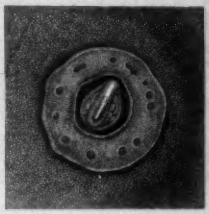

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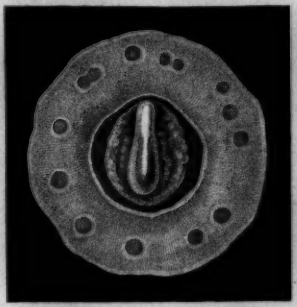

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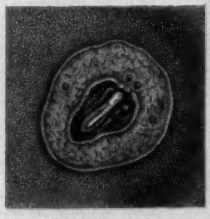

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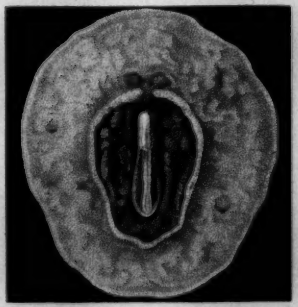

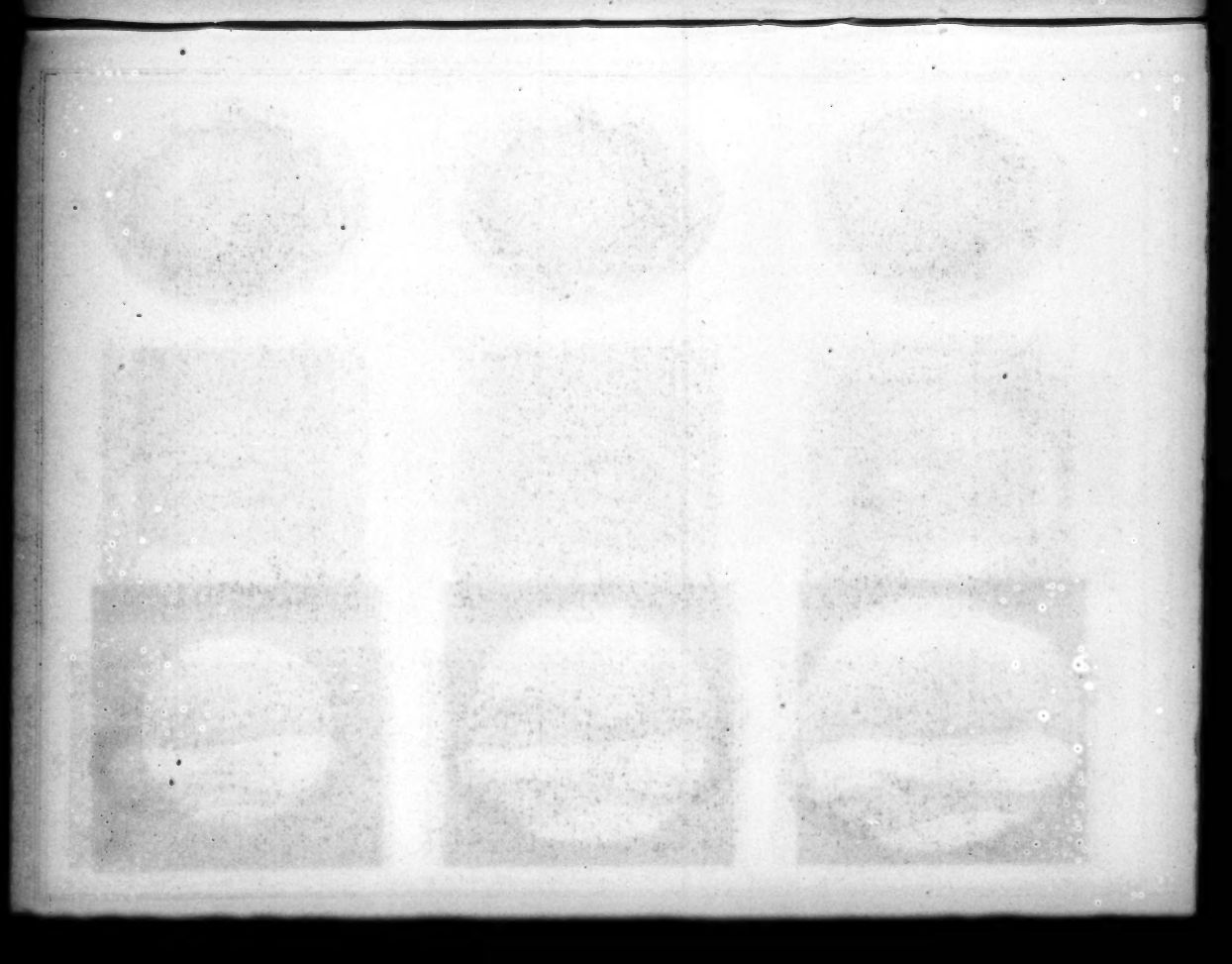

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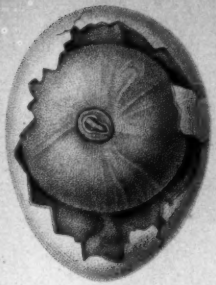

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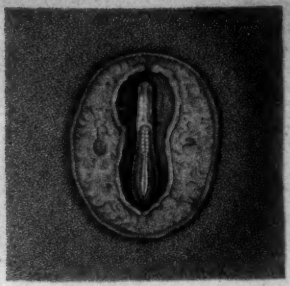

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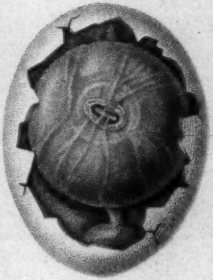

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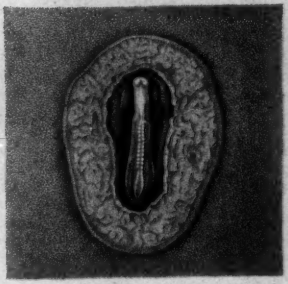

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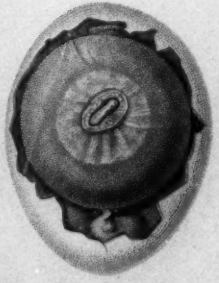

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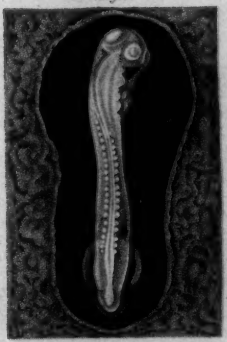

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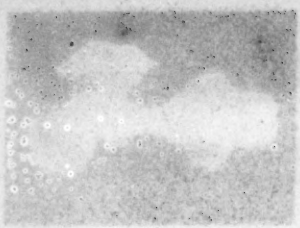

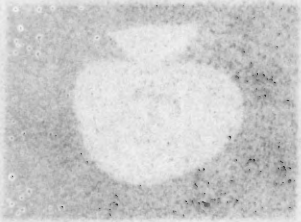

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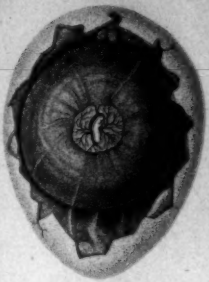

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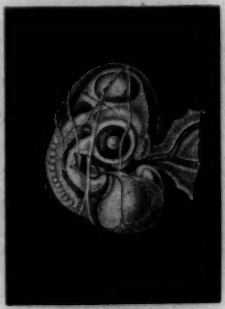

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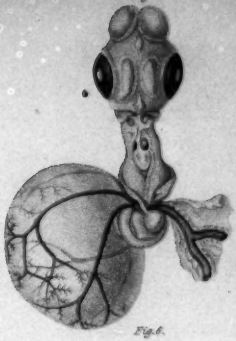

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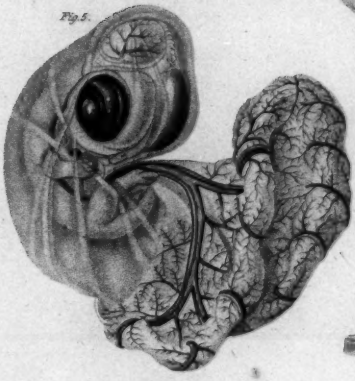

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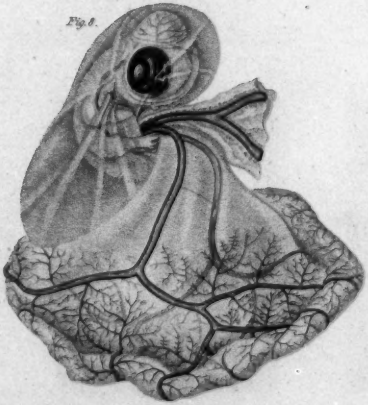

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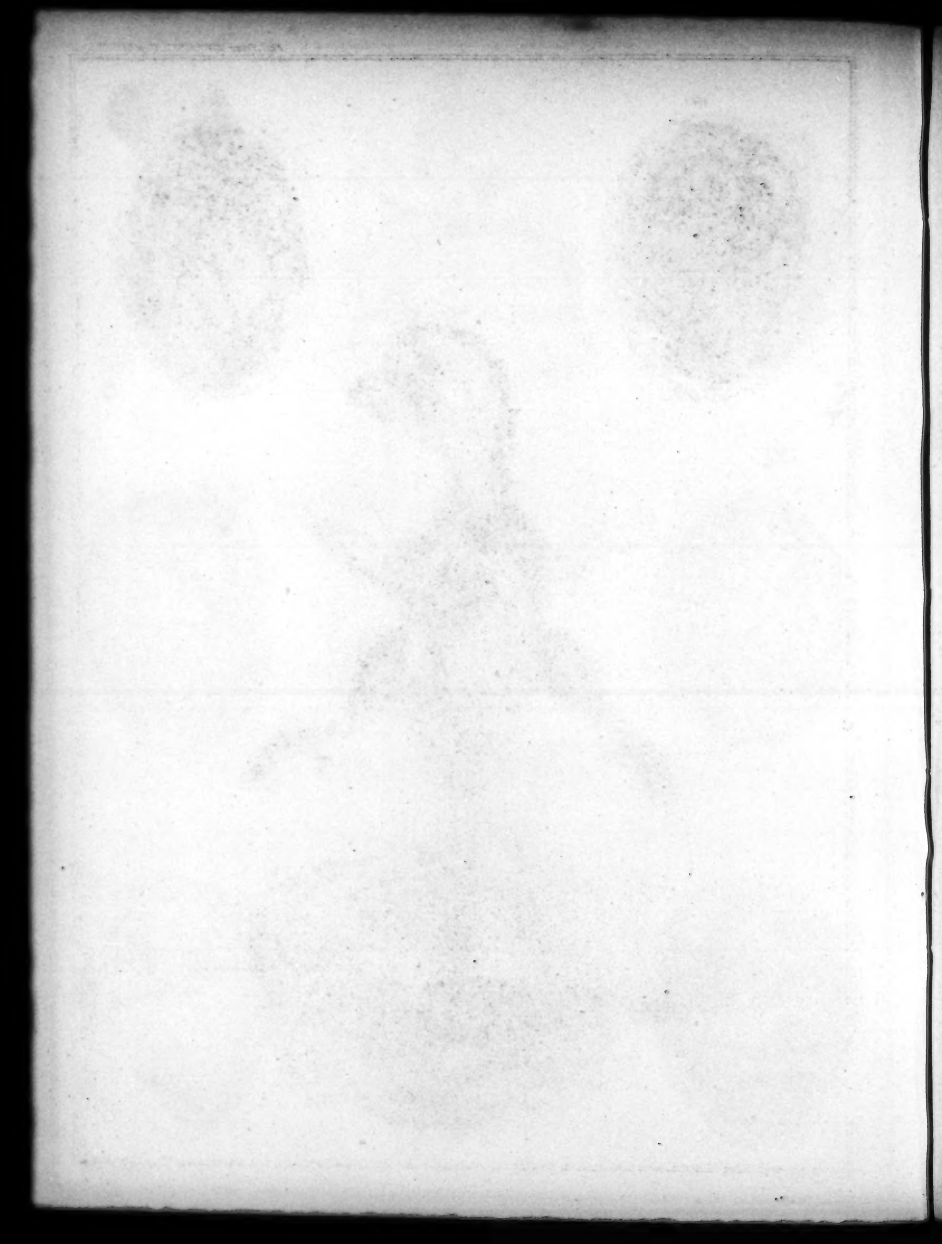

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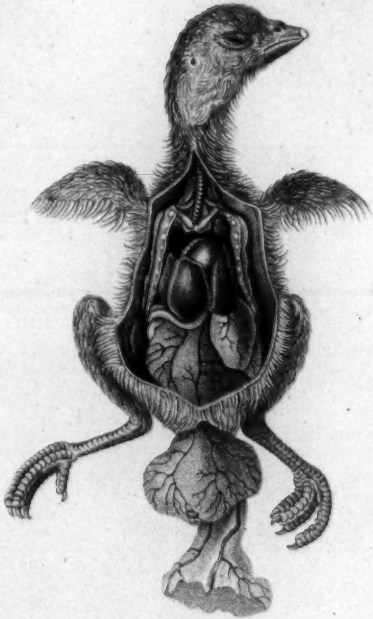

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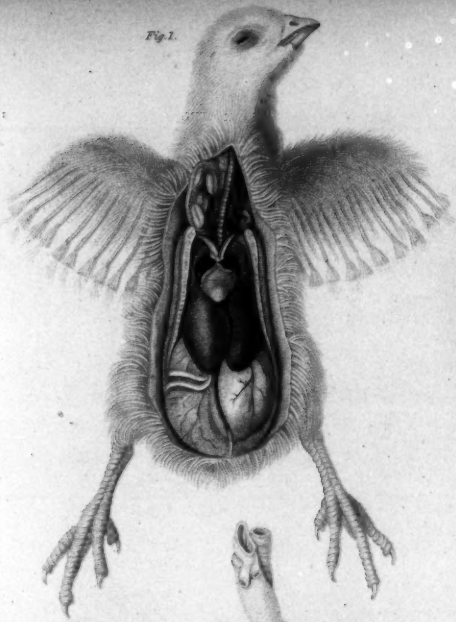

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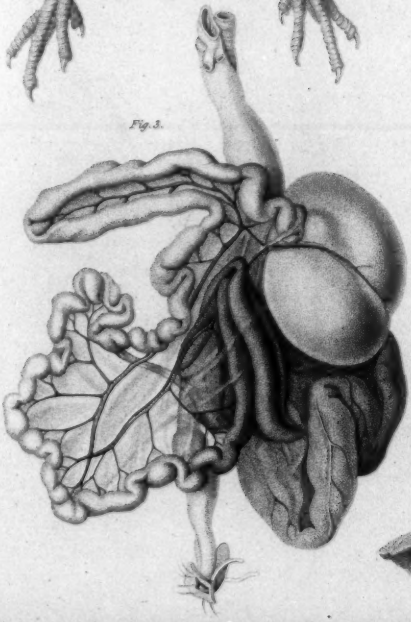

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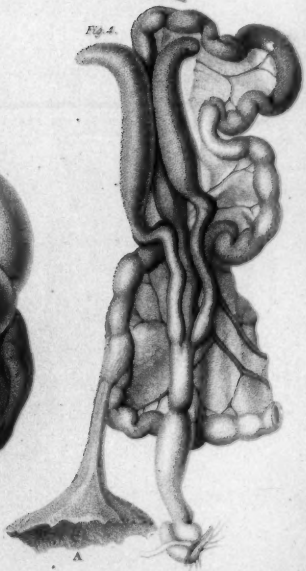

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XXVI. *Some observations on Corrosive Sublimate.* By JOHN DAVY, *M. D. F. R. S.*

Read June 6, 1822.

I AM not aware that the operation of light on corrosive sublimate, has yet been minutely considered. It is known that the *liquor hydrargyri oxymuriatis* of the London Pharmacopœia is decomposed by light; it has been stated, that the compound itself, when exposed to light, undergoes the same change; and it has been recommended, in consequence, to keep it in black bottles.

With a view to acquire some precise information on this subject, the following experiments were instituted.

A few grains of corrosive sublimate, in the state of fine powder, were exposed to sunshine for 14 days in a small glass tube, corked and sealed. No change was then produced, as was proved by the corrosive sublimate dissolving entirely in muriatic acid.

A solution of corrosive sublimate in distilled water was exposed to sunshine for the same length of time. A thin white crust formed, which was found to be calomel; and traces of free muriatic acid were detected in the solution.

Some *liquor hydrargyri oxymuriatis* and solutions of corrosive sublimate in rectified spirit and in ether were exposed to sunshine for the same time. In the former, a considerable crust

of calomel formed; whilst in the two latter no change took place.

Some oil of turpentine was poured on corrosive sublimate, and exposed to sunshine for a week. The fluidity of the oil was slightly impaired, but the corrosive sublimate was unaltered.

To a saturated aqueous solution of corrosive sublimate, a few drops of muriatic acid were added, and to another saturated solution, a small quantity of muriate of ammonia. No change was produced in these solutions by the action of light during exposure for three weeks.

From these experiments it may be deduced, that light alone has not the power of decomposing corrosive sublimate, and that it does not produce the effect, excepting when aided by affinities of a complicated nature.

In confirmation of this conclusion, I beg leave to relate some other experiments made with a view to illustrate it, and which, I trust, will not prove uninteresting in themselves.

The solubility of corrosive sublimate in water and alcohol is differently stated by authors. In one experiment, made with as much accuracy as was in my power, 37 grains of distilled water were required to dissolve 2 grains of corrosive sublimate at the temperature 57° FAHRENHEIT, which is in the proportion of about 5.4 per cent. Its degree of solubility increases greatly with the temperature, but in what ratio, it is not easy to ascertain with precision, owing to the trivial circumstances by which the crystallization of the salt is affected.

Alcohol, of specific gravity .816, at 60°, dissolves, I find, half its weight of corrosive sublimate: thus 10 grains of it

were completely dissolved in 20 grains of alcohol; one grain more was added and gentle heat applied; it dissolved, but on cooling separated in the form of minute spicular crystals. The saturated solution was of specific gravity 1.08.

Ether, I find, dissolves nearly one-third its weight of corrosive sublimate: thus, 20 grains of sulphuric ether of specific gravity .745, took up 7 grains. It was of the same specific gravity as the alcoholic solution, viz. 1.08. It may be worthy of notice, that the solvent power of ether, as well as I could judge from my experiments, is not increased by elevation of temperature, or diminished by its reduction. The boiling point, too, of the solution and of pure ether, appears to be the same. In the act of ebullition, the solution seems to be decomposed: where a bubble of ether is formed, there a minute portion of corrosive sublimate is precipitated, owing, probably, to the property just mentioned.

Though no change is produced in corrosive sublimate by oil of turpentine under the influence of light, a considerable action and mutual decomposition is the result of a mixture of the two being gently heated. On the application of a gentle heat to corrosive sublimate in fine powder moistened with oil of turpentine, the mixture becomes of a fawn colour; there is a sudden and considerable elevation of temperature, acid fumes are generated, calomel is formed, and if the heat be raised, a residue of carbon is obtained. The results appear to be modified by the proportions of the two substances: when the quantity of corrosive sublimate is large, the whole of the oil appears to be completely decomposed, and the products are, liquid muriatic acid, calomel, and charcoal: when the oil is in excess, the part that escapes decomposition, passes over

impregnated with muriatic acid; and, judging from its smell, appears to contain a minute quantity of artificial camphor. From the calomel that is mixed with it, and which rises with it, forming a very dense white vapour, it is not easy, on a small scale of experimenting, to demonstrate the actual formation of camphor.

I believe, that changes very similar take place, when corrosive sublimate is heated with other oils, both volatile and fixed. Of the latter, I have tried one only, oil of almonds; when heated with corrosive sublimate, it immediately blackens, and acid fumes are disengaged. Of the former, I have tried, in addition to the oil of turpentine, the oils of cinnamon, nutmeg, juniper, carraway, peppermint, and cloves. On the application of heat to the first four, mixed with corrosive sublimate, the colour changes to brown, and then to black, and copious acid fumes are disengaged. The mixture of oil of cloves and corrosive sublimate, when gently heated, becomes of a beautiful bright purple, and the heat being raised, acid fumes are disengaged, an oil of the same purple colour distils over, and the residue becomes black. The purple oil appears to be a compound of muriatic acid gas and oil of cloves; and it can be produced either by passing muriatic acid gas into oil of cloves, or by agitating the oil with liquid muriatic acid. On the addition of corrosive sublimate to oil of peppermint, the colour of the oil is immediately changed to bright yellow; on the application of heat, it instantly blackens; a light purple oil volatilises with acid fumes, and calomel with carbonaceous matter remains. The production of a purple oil in this instance, appears to depend on the same cause as in the preceding, and it can be

formed in the same way by the action of muriatic acid or its gas.

In a paper published in the Philosophical Transactions for 1812, I have noticed the affinity of muriatic acid for corrosive sublimate. Muriatic acid of specific gravity 1.158, at 74°, dissolves, I find, twice its weight of corrosive sublimate. This solution may be considered as composed of 11 proportions of water, 1 muriatic acid,* and 1 salt. In the act of forming, heat is evolved. At 74° this solution is of specific gravity 2.412. When its temperature is lowered a few degrees, it suddenly becomes solid, and forms a mass of delicate needle crystals, which rapidly melt, when the containing vessel is held in the warm hand.

It is commonly stated in systematic works, that corrosive sublimate is soluble in the sulphuric and nitric acids, as well as in the muriatic acid. From the experiments which I have lately made, this does not appear to be the case. One-tenth of a grain of corrosive sublimate was added to 50 grains of nitric acid of specific gravity 1.45; kept some time at the temperature 90°, it did not diminish in bulk, nor did it appear to dissolve even at the boiling point of the acid, nor did the acid appear turbid on cooling, nor were any crystals deposited. A similar experiment was made with the same quantity of sublimate and 68 grains of concentrated sulphuric acid: at 90° the sublimate did not dissolve; and on the application of heat, fumes appeared, the salt rose through the acid, and a delicate crust of it was formed in the cool part of the vessel.

The experiments which I have made, and which I shall immediately relate, tend to corroborate an opinion long ago

* New System of Chem. Phil. by John Dalton, vol. ii. p. 295.

entertained, that muriate of ammonia and corrosive sublimate are capable of uniting and of forming a double salt,* and to prove that similar compounds may be formed with some other muriates.

In the dry way, there appears to be an affinity exercised between corrosive sublimate and muriate of ammonia. A mixture of the two, in the proportions of 34 of the former, and 6.75 by weight of the latter, heated, forms a compound more fusible and less volatile than either ingredient separate; it may be kept liquid without volatilising by the gentle heat of a spirit lamp; on cooling, it exhibits a very light grey translucent mass of a faint pearly lustre; strongly heated, it sublimes, and appears to be partially decomposed, as traces of calomel and free muriatic acid are found mixed with the sublimate. This compound, formed of one proportion of each ingredient, has more the character of a chemical compound, than any other mixture of the two ingredients that I have tried.

In the moist way, the affinities of corrosive sublimate and muriate of ammonia are better marked, and some of the combinations of the two have tolerably well defined qualities. The following have the best claim to be considered distinct combinations of any which I have yet made:

No.	Water.	Muriate of Ammonia.	Corrosive Sublimate.
1	9.00 grs.	6.75	34.00
2	9.00	3.37	17.00
3	9.00	3.37	8.50
4	9.00	10.12	25.50

* The sal-alembroth of the alchemists was a compound of this kind.

No. 1, is liquid at 140°; on cooling, it forms a solid mass of needle crystals. No. 2, is liquid at 85°, and solid at 55°. In the liquid form at the temperature just mentioned, it is of specific gravity 1.98. No. 3, is liquid at 55°, and of specific gravity 1.38. No. 4, is liquid at about 105°; on cooling slowly to 60°, it deposits some crystals which are four-sided prisms, composed of facets alternately broad and narrow.

That corrosive sublimate and muriate of ammonia have a strong affinity for each other, is evident from the circumstance, that united, the solubility of the compound exceeds that of the most soluble ingredient. This is proved by the solubility of the 3d compound just described: farther in proof, it may be mentioned, that a saturated solution of muriate of ammonia at 60°, is capable of dissolving its own weight very nearly of corrosive sublimate, and that after this addition it is capable of taking up more muriate of ammonia; thus, 25.8 grains of such a saturated solution, after having taken up 25.1 grains of corrosive sublimate, dissolved 7 grains more of muriate of ammonia. From this experiment it would appear, that corrosive sublimate is about 17 times more soluble in a saturated solution of muriate of ammonia than in water, and not 30 times, as is stated by some authors.

The results of these experiments led me to make trial of some other muriates, as of baryta, magnesia, potash, and soda.

A saturated solution of muriate of baryta, formed of 20 grains of water and of 8.7 grains of the crystallized salt,* dissolved 16 grains of corrosive sublimate at 60°, and 4 grains

* 8.7 grains of this salt, heated nearly to redness, lost 1.4 grain water of crystallization.

more when gently heated; on cooling, a very few granular crystals were deposited. The solution was of specific gravity 1.9. After rest for several hours, it deposited a small number of minute and apparently cubical crystals.

31 grains of muriatic acid, of specific gravity 1.18, carefully neutralized with magnesia, dissolved 40 grains of corrosive sublimate, and, when gently heated, 25 grains more. This solution remained transparent on cooling. When 5 grains more of the sublimate were added, these too were dissolved when heated; on cooling, a good many spear-shaped crystals formed. The solution first made, when poured from one vessel into another, had an oily appearance, and was rather less fluid than concentrated sulphuric acid; it was of specific gravity 2.83; gently evaporated, greyish pellicles formed, which rapidly deliquesced on exposure to the air.

A saturated solution of muriate of potash formed of 21 grains of water and 7 grains of dry muriate of potash, gently heated, dissolved 8 grains of corrosive sublimate. On cooling to 60°, it deposited only a very few needle crystals; but when cooled by means of ether to 50°, it became nearly solid, a mass of delicate needle crystals admitting of being inverted.

A saturated solution of common salt, composed of 20 grains of water and 7 of salt, dissolved 32 grains of corrosive sublimate at 60°; gently heated, 3 grains more were dissolved, and remained in solution on cooling; but, on a farther addition of corrosive sublimate, the solution formed by heat, deposited, on cooling, small rhomboidal crystals. The solution, containing 35 grains of sublimate, was of specific gravity 2.14. Like muriate of ammonia, the solubility of common

salt in water appears to be increased by combining with corrosive sublimate, but in a less degree.

It appeared probable, that a compound of common salt and corrosive sublimate might be formed in the dry way. 7.5 grains of the former and 34 of the latter were heated together: no proof was afforded of combination having taken place; the corrosive sublimate, on the application of heat, rose as readily as if heated by itself.

I thought it possible that water containing common salt, might have the power of dissolving chlorides that are insoluble in water alone; but experiment did not confirm the conjecture in the instance of calomel and horn silver.

May not the compounds of corrosive sublimate and common salt, muriate of magnesia and baryta, respectively, be considered as constituted of one proportion of each ingredient? The definite nature of the compounds with muriate of ammonia and potash, are perhaps more questionable.

It is worthy of remark, that all these compounds exhibit the properties of the most active constituent, or of that, the saturating power of which is greatest; so that, though the quantity of corrosive sublimate dissolved in any one instance is large, it modifies very little the character of the solution.

To conclude; it would appear from the preceding experiments that these menstrua, which have a strong affinity for corrosive sublimate, prevent its decomposition when exposed to light, as the muriates, alcohol, and ether; and, on the contrary, that those solvents which exercise a weak affinity on it, and have a stronger affinity for muriatic acid, as water, and exceedingly dilute alcohol, aid the decomposing power of light.

The practical application to be deduced, relative to the formula for the *liquor hydrargyri oxymuriatis*, is obvious, and does not require to be pointed out.

Fort Pitt, Chatham,
May 14, 1822.

XXVII. *On the state of water and aëriform matter in cavities found in certain crystals.* By Sir HUMPHRY DAVY, *Bart. P. R. S.*

Read June 13, 1822.

THERE are few enquiries in natural science more calculated to awaken our curiosity, than those relating to the changes which the matter composing the surface of our globe has undergone. The imagination is excited by the magnitude of the operations, by the obscurity of the phenomena, and the remoteness of the time at which they occurred; and all the intellectual powers are required to be brought into activity to find facts or analogies, or to institute experiments, by which they may be referred to known causes.

The crystallizations constituting the whole of the rocks which are usually called primary, and those found in such abundance, even in the rocks which are termed secondary, prove that a considerable part of the materials of the surface of the globe must have been either fluid or aëriform; for these are the only states from which the regular arrangements of the molecules of bodies constituting crystals, can be produced.

Geologists are generally agreed, that the greater number of the crystalline mineral substances must have been previously in a liquid state; but different schools have supposed different causes for their solution; some attributing this effect principally to the agency of water, others to that of heat.

When, however, it is considered, that the solvent power of water depends upon its temperature, and its deposition of solid matters upon its change of state or of temperature; and that being a gravitating substance, the same quantity must always belong to the globe, it becomes difficult to allow much weight to the arguments of the Wernerians or Neptunists, who have generally neglected, in their speculations, the laws of chemical attraction.

There are many circumstances, on the contrary, favourable to that part of the views of the Huttonians or Plutonists, relating to the cause of crystallization; such as the form of the earth, that of an oblate spheroid flattened at the poles; the facility with which heat, being a radiating substance, may be lost and dissipated in free space; and the observations which seem to show the present existence of a high temperature in the interior of the globe.

I have often, in the course of my chemical researches, looked for facts or experiments, which might throw some light on this interesting subject, but without success, till about three years ago; when, in considering the state of the fluid and aëriform matters included in certain crystals, it appeared to me, that these curious phenomena might be examined in a manner to afford some important arguments as to the causes of the formation of the crystal.

It is well known that water, and all fluids at usual temperatures, are more expansible by heat than siliceous or other earthy matters; and supposing these crystals to have been formed, and the water or fluid inclosed in them, at a pressure and temperature not very unlike those of our existing atmosphere, this fluid ought to fill nearly the same space as when

included, and the elastic fluid confined with it, supposing it non-absorbable, ought to be in the same state of density. On the contrary, if the earthy matter and the fluid separated from each other under a much higher temperature than that now belonging to the surface, a certain vacuum might be expected in the cavity from the contraction of the fluid, and if any gas were present, a considerable rarefaction of it; and though, supposing a much higher temperature on the surface of the globe, the atmosphere formed by aqueous vapour must have had much greater absolute weight, which, as liquids are *compressible*, must have influenced the volume of the fluid at the time it was inclosed, a circumstance which would render it impossible to draw any conclusion as to the exact temperature, yet still the experiments appeared to offer, on any view, interesting results; and I was the more desirous of performing them, as I believe the nature of the fluid and aëriform matters included in rock crystals and other siliceous stones, has never been accurately ascertained.

Having purchased some crystals, and having had others committed to my care by the liberality of my Brother Trustees of the British Museum, and of my friend Professor BUCKLAND, I proceeded to make the necessary experiments upon them. It will be improper for me to take up the time of the Society by a minute description of my manipulations. Holes were drilled in the crystals by the use of diamonds, generally by Mr. NEWMAN, under distilled water, or mercury, the gas was expelled by the introduction of wires, and the fluids included in the cavities were drawn out by means of fine capillary tubes, and experiments were afterwards made to determine the space they occupied, which had been accurately measured and

marked upon the crystal. The chemical nature of the fluid and gas was determined by processes which were necessarily difficult from the smallness of the quantities operated upon; but which are too well known to the chemical philosophers of this Society to need description.

The first three crystals that I examined were from Schemnitz, in Hungary; the cavities that they contained were proved not to be permeable to the atmosphere, by exposure to rarefied air, alone, and under water, in the receiver of an air pump, a circumstance which it was necessary always to attend to, in order to render the experiment availing.

A cavity in one of the crystals was pierced under oil, three under distilled water, and one under mercury. In all of them the fluid rushed in when the cavity was opened, and the globule of elastic fluid contracted so as to appear from six to ten times less than before the experiment. The fluid in all the crystals (in two it was minutely examined) was found to be water nearly pure, containing only a minute portion of the alkaline sulphates. The elastic fluid, as well as I could ascertain from the very minute quantities I could procure, appeared to be azote, unmixed with any other substance.

The largest cavity, which was in the crystal put into my hands by Professor BUCKLAND, contained a space equal to 74.5 grains of mercury; the water in it equalled in volume 48.1 grain measures of mercury; and the globule of air, after the experiment, equalled in diameter a globule of mercury weighing 4.2 grains, so that the elastic fluid had contracted at least between six and seven times.

In the other experiments, the cavities being much smaller, the quantities of air and fluid could not be accurately mea-

sured; but there seemed to be nearly the same relation between the space filled by fluid, and that containing aëriform matter; and in all of them the contraction of the globule of aëriform matter was evidently greater, and in one instance to less than $\frac{1}{10}$ of its original bulk.

The fourth crystal that I experimented upon was of unknown locality; but I have reason to believe that it was from Guanaxuato, in Mexico, as it strongly resembled some that Mr. HEULAND showed me from that place. The cavity in it was extremely small, and when pierced into, under distilled water, the globule of gas, from being $\frac{1}{8}$ of an inch in diameter,* diminished so as to be less than $\frac{1}{24}$; so that its rarefaction was much greater in this than in the other instances; the water was too small in quantity to be minutely examined; it seemed to be nearly pure, producing a cloudiness barely perceptible in solutions of nitrate of silver and muriate of baryta.

It was an interesting point to ascertain whether the same circumstances occurred in productions found in rocks which have been generally considered as of igneous origin, such as the basaltic rocks in the neighbourhood of Vicenza, the chalcedonies of which so often afford included water. I found it much more easy to make experiments of this kind, and to procure specimens, which were abundantly supplied to me from the same sources as those I have just referred to; and though some of these specimens proved to be permeable to the atmosphere, and to have been filled with water artificially,

* I have not thought it necessary to refer to the heights of the barometer and thermometer in these experiments, as it is impossible to gain any other than general results, upon quantities in which differences arising from atmospheric temperature and pressure, would be quite unappreciable.

yet many occurred, in which the sides of the cavity were absolutely impervious to air or water.

The results that I obtained were very analogous. Water containing very minute quantities of saline impregnations, occasioning barely a visible cloudiness in solutions of silver and of muriate of baryta, was found to be the fluid; the gas was azote, but it was in a much more rarefied state than in the rock crystals, being between 60 and 70 times as rare as atmospheric air.

The quantity of water was to the void space in greater proportion than in the rock crystals. In the instance in which the most accurate experiment was made, namely, on the great specimen preserved in the collection of the British Museum, and which weighed 380 grains, the quantity of water was 29.9 grains, the space occupied by aëriform matter was equal to 11.7 grains of water, the volume of the globule of gas at the common pressure was to that of its rarefied volume as 1 to 63.

It occurred to me that atmospheric air might have been originally the elastic fluid included in these siliceous stones and in the crystals, and that the oxygene might have been separated from the azote by the attraction of the water, and a direct experiment seemed to confirm this idea. A chalcedony which had been bored was placed in water free from air under a receiver, which was exhausted till a portion of gas from the interior of the crystal had escaped into a proper receptacle. This gas examined by nitrous gas, was found to contain nearly as much oxygene as atmospheric air; so that there is every reason to believe that the water had emitted oxygene during the exhaustion.

I endeavoured to find some calcareous secondary rocks, or crystals belonging to them, containing cavities, on which experiments of the same kind might be made; but in a number of trials I have as yet found none impermeable to the atmosphere; and the cavities of such, when bored, are always found to contain atmospheric air in a common state of density.

I was surprised to find that this was the case even with cavities in calcareous spar in the centre of a lime-stone rock; yet these cavities which contained atmospheric air did not fill with water when the stone was placed in water under an exhausted receiver. When however it was dry, and placed in a receiver alternately exhausted and filled with hydrogene, the air that was produced by piercing the cavities, was found mixed with hydrogene; proving that the substance of the stone was permeable to elastic fluid.

I hope soon to be able to make further researches on this subject; but in reasoning upon the vacuum, or rarefied state of the aëriform matter in the cavities of these rock crystals and chalcedonies, it appears difficult to account for the phænomenon, except on the supposition of their being formed at a higher temperature than that now belonging to the surface of the globe; and the most probable supposition seems to be, that the water and the silica were in chemical union, and separated from each other by cooling.

Water in the temperature of the arctic winter is constantly a crystallized body. As a fluid, its solvent powers are increased as its heat becomes higher, and when elastic, the density of its vapour is exalted in proportion to its heat; so that an atmosphere of steam, supplied from an indefinite

source above water, would render it capable of receiving a very high degree of heat. Lime retains water in combination at a heat above 250° FAHRENHEIT; baryta retains it (even under ordinary pressures) at a strong red heat, and fuses with it. It is extremely likely that a liquid hydrate of silica would exist, under pressure, at high temperatures; and like all liquid bodies in the atmosphere, would probably contain small quantities of atmospheric air; and such a supposition only is necessary to account for the phænomena presented by the water in rock crystal and chalcedony.

As, however, steam or aqueous vapour may be considered as having a share in these results, if it be supposed included in the cavity, no exact conclusions can be drawn from the apparent degree of contraction of the water; particularly as the late ingenious researches of Mr. PERKINS show, that water is much more compressible than was formerly imagined; and the volume of water, however high its temperature, must be influenced by the pressure to which it is exposed; so that a certain compressing weight may not only impede, but altogether counteract the expansive force of heat.

Many speculations might be indulged in on this subject, but I shall not at present enter upon them; and I shall conclude by observing, that a fact, which has been considered by the Neptunists, above all others as hostile to the idea of the igneous origin of crystalline rocks, namely, the existence of water in them, seems to afford a decisive argument in favour of the opinion it has been brought forward to oppose.

June 1, 1822.

APPENDIX.

SINCE the foregoing pages were communicated to the Royal Society, I have made some new experiments on the same subject; all of them, except two, offered results of the same kind as those I have detailed, and upon such I shall not enter: but these two, from their peculiarity, will not, I trust, be thought unworthy of a particular notice.

In examining, with Mr. HEULAND, the beautiful specimens of rock crystals in the collection of CHARLES HAMPDEN TURNER, Esq. I observed one crystal which, Mr. HEULAND informed me, was from La Gardette, in Dauphiné, that contained a considerable cavity, in which there was a viscid brownish liquid, resembling in its appearance and consistence linseed oil. As the void space or cavity filled with aëriform matter appeared considerable in proportion to the fluid, I expressed a desire to pierce the crystal; and Mr. TURNER, hearing of my wish, was so kind as to gratify it in the most polite and liberal manner, by presenting to me the specimen. With Mr. NEWMAN's assistance I made the usual experiments upon it. The cavity was pyramidal, and nearly the third of an inch in diameter. I soon ascertained that the fluid was not water, as it congealed and became opaque at a temperature of 56°. When the crystal was pierced under distilled water, the water rushed in and entirely filled the cavity, so that no other aëriform matter but the vapour of the substance could have been present; the water was rendered white and cloudy, apparently by the substance. I endeavoured to collect some of it for chemical examination, but it was too small in quantity (not equalling in volume $\frac{1}{6}$ of the volume of the cavity,) to be submitted to analysis. It swam on the water, had

no distinct taste, but a smell resembling naphtha; a portion of it taken out mixed with the water, when exposed to heat acted like fixed oil, and it seemed to have a high temperature of ebullition. It inflamed, producing a white smoke.

The fact, of almost a perfect vacuum existing in a cavity containing an expansible but difficultly volatile substance, may be considered as highly favourable to the theory of the igneous origin of crystals: the other experiment is of a nature entirely different, though its result *may* be explained in the same supposition.

In examining a crystal in the collection of the Royal Institution, and which from its characters I believe to be from Capaó d'Olanda, Province of Minas Geraes, Brazil, I observed that the quantity of aëriform matter was unusually small in proportion to the quantity of fluid, in two or three cavities not occupying $\frac{1}{10}$ or $\frac{1}{12}$ of the space; and from the peculiarity of its motion, it appeared to be more likely to be compressed, than rarefied elastic fluid; and in piercing the sides of the cavities I found that this was the case; it enlarged in volume from ten to twelve times; the fluid was water, but the gas was too minute in quantity to be examined.

It will be interesting to ascertain under what circumstances, and in what situations crystals of this kind are found. If they be supposed of igneous origin, they must have been formed under an immense weight of atmosphere or fluid, sufficient to produce a compression much more than adequate to compensate for the expansive effects of heat, a supposition which, in consequence of Mr. PERKINS's experiments, already alluded to, may be easily formed.

July 6, 1822.

XXVIII. *Some experiments on the changes which take place in the fixed principles of the egg during incubation.* By WILLIAM PROUT, *M. D. F. R. S.*

Read June 20, 1822.

IN the year 1816, I was induced to commence a series of experiments on the egg during incubation, with the view of ascertaining the nature of the changes which take place during that process. My inquiry was chiefly limited to the fixed principles, namely, the earthy and saline matters; but my attention was more particularly directed to the source whence the earthy matter, constituting the skeleton of the chick, was derived.

With these views, the egg was analyzed in its recent and unaltered state, and at the end of the first, second, and third weeks of incubation. My experiments were chiefly confined to the eggs of the domestic fowl, but have been likewise partially extended to those of the duck and turkey. The investigation has been renewed, and the experiments repeated at various intervals since the period above mentioned; but the difficulty of the subject, and various accidents, have prevented me from completing them till the present time; and the results, which, after all, are much less perfect than I could wish, I have now the honour of submitting to the Society.

Preliminary experiments on the egg in its recent and unaltered state.

The specific gravity of new laid eggs has been found to vary from 1.080 to 1.090. When kept for some time, eggs, as is well known, rapidly lose weight, and become so specifically light as to swim in water. This diminution of specific gravity, however, is only apparent, and depends on the substitution of air for a portion of the water of the egg which escapes; for it is obvious, that the specific gravity of the constituent principles of the egg must be rather increased than diminished by the escape of water. The following table shows the gradual loss of weight of an egg during a period of two years.

The original weight on the 19th May, 1820, the day it was laid, was 907.5 grains.

	Grains.	Loss per day.		Grains.	Loss per day.
19 May, 1820.	907.5		5 May, 1821.	648.7	.59
20 . . .	906.5	1.00	6 . . .	647.8	.90
24 . . .	901.3	1.30	5 December	488.2	.75
31 . . .	894.2	1.01	7 . . .	486.6	.80
8 June . .	886.6	.95	21 March, 1822.	418.5	.70
17 . . .	879.3	.81	25 April .	384.6	.82
27 . . .	870.7	.86	26 . . .	383.7	.90
19 July . .	848.5	1.01	17 May . .	365.2	.84
7 August .	829.6	.99	18 . . .	364.3	.90
26 . . .	810.8	.99	19 . . .	363.2	1.10
30 September	778.5	.92	Total Loss	544.3	.744 Mean

Hence we learn, that a moderately sized egg loses on an average about .75 grain in twenty-four hours, and that

uniformly during a very long period.* The loss appears to be somewhat greater in summer than in winter, owing, doubtless, to the difference in temperature, which, in the present instance, varied from 40° to 70°. On being broken, the whole of the contents of this egg were found collected at the smaller extremity in a solid state, but on being put into water, they absorbed a large portion of that fluid, and assumed an appearance not much unlike those of a recent egg; the smell also was perfectly fresh.

The relative weights of the shell, albumen, and yelk of different eggs are very different. With the view of investigating this point, and of obtaining something like an average, the following experiments were made. The eggs were boiled hard in distilled water, and the different parts weighed immediately in their *moist* state.

	Shell and Membrane.	Albumen.	Yelk.	Total.
	Grains.	Grains.	Grains.	Grains.
	80	394.3	289	763.3
	108	593	273.5	974.5
	107.3	575.8	236.2	919.3
	71.5	516.5	215	803
	103	503.7	269.3	876
	107	515.3	273.4	895.7
	93.2	605.5	252.4	951.1
	92.7	515.7	257	865.4
	96.8	510.6	210.8	818.2
	77.6	567.4	241.5	886.5
Mean	93.7	529.8	251.8	875.3

If we suppose each of these eggs to weigh one thousand

* If the average of the above means be taken, the loss per day will be about .9 grain.

grains, the weights of the constituent principles of each, when reduced to this common standard, will be as follow:

	Shell and Membrane.	Albumen.	Yelk.
	Grains.	Grains.	Grains.
	104.8	516.6	378.6
	110.8	608.5	280.7
	116.7	626.3	257.0
	89.0	643.2	267.8
	117.6	575.0	307.4
	119.5	575.3	305.2
	98.0	636.6	265.4
	107.1	596.0	296.9
	118.3	624.0	257.7
	87.5	640.0	272.5
Average	106.9	604.2	288.9

Hence, if we suppose a recent egg to weigh one thousand parts, the relative proportions of the shell, albumen, and yelk, will be as 106.9, 604.2, and 288.9; and for the sake of easier comparison in all the subsequent experiments, the numbers are reduced to the above standard, or to the supposition, that the original weights of the eggs employed were, when just laid, exactly 1000 grains.

When an egg is boiled in water, it loses weight, particularly if it be removed from the water when boiling, and be permitted to cool in the open air;* the water also on examination will be found to contain a portion of the saline contents of the egg. The loss of weight from boiling is by no means constant, but has been found to vary from 20 to 30 grains, on

* When permitted to cool in water it sometimes gains a little in weight, owing to the absorption of water. Eggs placed in a strong solution of common salt are said to become highly saline throughout. This mode has been recommended for preserving them, but I have never tried the experiment.

the supposition, that the original weights of the eggs employed were 1000 grains. On the same supposition, also, it has been found, that the quantity of saline matter obtained by evaporating to dryness the distilled water in which an egg has been boiled, amounts, at an average, to about .32 grain. This saline residuum is strongly alkaline, and yields traces of animal matter, sulphuric acid, phosphoric acid, chlorine, an alkali, lime and magnesia, and carbonates of lime and magnesia; in short, of almost every principle existing in the egg. The carbonate of lime, however, is generally most abundant, and is obtained by evaporation in the form of a fine powder.

The shells of eggs have been analyzed by VAUQUELIN* and MERAT GUILLOT;† but these chemists seem to have over-rated the quantity of animal matter, and of phosphate of lime contained in them. When shells which had been dried in vacuo at 212°, were dissolved in dilute muriatic acid, the quantity of animal matter obtained was only about 2 per cent. while the quantity of phosphates of lime and of magnesia never amounted to quite 1 per cent.; the rest was carbonate of lime mixed with a little carbonate of magnesia. When burnt, egg-shells, as VAUQUELIN has observed, yield traces of sulphur and iron.

The *membrana putaminis*, on the supposition that the origi-

* Annales de Chimie, tom. 29 et 81.

† Ibid. tom. 34. It is probable that the different results obtained by these chemists depended, in a great degree, on the different mode in which the experiments were made. The phosphate of lime present in egg-shells is apparently connected with the animal matter, and when the latter is destroyed by combustion, the whole quantity present will of course be obtained. The quantity of animal matter present also, being in this mode of analysis necessarily estimated from the loss of weight occurring during the process, must appear greater than it ought to do, because part of this loss will obviously depend on the escape of water.

nal weight of the egg be 1000 grains, weighs, when dried in vacuo at 212°, about 2.35 grains; and on being burnt, yields traces of phosphate of lime.

It may be observed here, that the great differences in the quantities of the earthy matter existing in the shells of different eggs, have rendered the average totally inapplicable in these experiments, as will be shown hereafter; hence, a more detailed analysis of this part of the egg was deemed unnecessary.

Saline contents of the recent egg.

As my attention at present is chiefly confined to the fixed principles of the egg, I shall not here enter on a description of its immediate principles, which will be found sufficiently detailed in all the modern chemical treatises. The saline principles, however, particularly of the yelk, have been less minutely examined; hence, it becomes necessary to relate the manner in which the following analyses were conducted; and here it may be premised generally, that all the results were obtained by combustion; and that the following observations are to be understood as applicable to the whole of the experiments subsequently related in this inquiry.

The *albumen* burns with difficulty, unless care be taken to remove the saline matter by frequent washings; but if this point be attended to, the whole of the carbonaceous matter may be burnt off even in a covered crucible. In the subsequent experiments, the saline and earthy matters were removed from the crucible after combustion by distilled water; a little ammonia was then added, and the whole permitted to remain at rest for twenty-four hours; the clear solution containing the alkaline salts was now carefully poured off, and the insoluble residuum, consisting of the phosphate of lime and triple phos-

phate of magnesia and ammonia, after being washed with distilled water, was dried and weighed. The alkaline solution, together with the washings of the earthy phosphates, were then evaporated to dryness, and exposed to a low red heat; and the weight of the saline residuum being accurately noticed, the whole was again dissolved in distilled water. A few drops of nitric acid being now added to neutralize the excess of alkali present, nitrate of barytes was dropped into the solution as long as any precipitate fell. The precipitate was obtained by decanting off the solution as before, and, after being well washed, its weight ascertained: from this the quantity of sulphuric acid present was determined by calculation.* To the solution, thus freed from sulphuric acid, nitrate of barytes, and afterwards ammonia, were added. The phosphate of barytes thus obtained was collected, washed and weighed as before, and the quantity of phosphoric acid present obtained by calculation.† Nitric acid was again added in slight excess to the original solution, and nitrate of silver dropped into it as long as any precipitate fell; from the chloride of silver obtained, the quantity of chlorine present was estimated.‡ Lastly, the weights of the sulphuric and phosphoric acids and chlorine were added together, and their amount subtracted from the weight of the alkaline residuum formerly obtained by evaporation, the remainder, of course, indicated the quan-

* On the supposition that the weight of the atom of sulphuric acid is 50, and that of barytes 97.5, oxygen being 10.

† On the supposition that the weight of the atom of phosphoric acid is 35, that of oxygen being 10.

‡ On the supposition that the weight of the atom of chlorine is 45, and of silver 137.5, that of oxygen being 10.

tity of potash and soda,* and carbonates of potash and soda present. Finally, the proportion of the earthy phosphates to one another was determined, and the quantities of the bases and acid obtained by calculation.

The *yelk* of the egg is exceedingly difficult of combustion; and indeed without proper precautions cannot be burnt at all, on account of the large quantity of phosphorus it contains; which, by undergoing a partial combustion, forms a glassy coating that effectually excludes the contact of the air from the coal, and prevents its farther combustion. After a variety of attempts, the following were the two methods employed. The yelk of an egg which had been boiled hard, and dried by exposure to the air, was rubbed in a mortar with a quantity of bicarbonate of potash. The mixture was then introduced into a platina crucible and exposed to a strong red heat, till the flame had ceased to escape from a small hole in the lid. The crucible being now removed from the fire, its contents, when cold, were again pulverised in a mortar with nitre. This mixture was then introduced a little at a time into the covered crucible till the whole was burnt. To the residuum distilled water was added, which of course took up every thing but the earthy phosphates, which were separated and weighed, while the alkaline solution, like that before mentioned obtained from the albumen, was submitted to the action of the appropriate re-agents, and thus the quantities of the different acids present ascertained. In this manner every thing was determined, except the proportion of alkaline matter present; and to ascertain this, other experi-

* The quantity of soda equivalent to the sodium in union with the chlorine, was determined by calculation.

ments with different yelks were made, in which lime and nitrate of lime were substituted for the bicarbonate and nitrate of potash.

With respect to the modes in which the different fixed principles originally exist in the egg, it is very probable, as BERZELIUS has remarked, that the sulphuric acid obtained from albumen is a product of combustion, and exists in it naturally as sulphur. The same also appears to be the case, to a great extent at least, with respect to the phosphoric acid, especially that obtained from the yelk. The chlorine seems to exist originally in union with sodium, forming common salt. As to the earthy principles, BERZELIUS is of opinion that their metallic bases are probably to be considered as constituent principles of the primary animal compounds. These circumstances have induced me to state the quantities of the acids obtained separately from the bases.

It may be also remarked here, that as the following experiments were made almost entirely with the view of comparison only, my object was rather to conduct them in some general and uniform manner, than to enter into any very minute discriminations, which did not appear to be immediately necessary to my purpose. For this reason, the proportion of the potash to the soda, and the exact quantity of carbonic acid combined with them, were not attempted to be determined. With the same view also, the proportion of the lime to the magnesia, though ascertained, was not expressed, but the united weights of both introduced into the column. Lastly, every one acquainted with chemistry will perceive, that from the mode of operating, the weights of the different principles will be somewhat underrated.

The relative proportions of the saline principles of different eggs vary in some instances considerably.* The three following are selected from a variety of other analyses as examples: the weight of each egg being reduced for the sake of comparison to 1000 grains.

No. 1.†

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Albumen	.29	.45	.94	2.92	.30
Yelk -	.21	3.56	.39	.50	.68
Total -	.50	4.01	1.33	3.42	.98

No. 2.

Albumen	.15	.46	.93	2.93	.25
Yelk -	.06	3.50	.28	.27	.61
Total -	.21	3.96	1.21	3.20	.86

* The most remarkable variations occur in the quantities of the sulphuric acid and chlorine. The tables exhibit instances of these, but I have met with still more striking anomalies, for which I was unable to account. I have sometimes thought these differences, as well as some other singular ones observed with respect to the earthy matters, might be connected with the sex of the future bird, but as no proof of this could be obtained, the results have been suppressed, on the supposition that they arose from some error in conducting the experiment. The three analyses given may be considered as average results. It may be also observed, that besides the above principles, iron is met with in almost all products of combustion; and the quantity in the egg, as the process of incubation proceeds, apparently increases considerably; but it was found impossible to ascertain its quantity with any degree of precision.

† The numbers in this and the following tables were obtained by calculation. In general, I did not weigh nearer than $\frac{1}{10}$th of a grain, but as the substances weighed were compounds, it was thought that the calculations of their constituent principles might be safely carried to the second decimal figure.

No. 3.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Albumen	.18	.48	.87	2.72	.32
Yelk -	.19	4.00	.44	.51	.67
Total -	.37	4.48	1.31	3.23	.99

Although the consideration of the immediate principles of the egg does not fall within my present design, yet I cannot refrain from giving the following analysis of the yelk of the recent egg.

The egg from which the yelk had been taken, which is the subject of the following experiment, had been boiled hard in distilled water, and the yelk, in its moist state, was found to weigh 316.5 grains. It was then partially dried by exposure to the air for several weeks; and to remove the remainder of the water was reduced to powder, and exposed to a temperature of somewhat more than 212°. The total loss of weight was 170.2 grains, which was supposed to indicate the quantity of water present. The remainder was now digested repeatedly in alcohol of specific gravity .807, till that fluid came off colourless. The residuum was perfectly white and pulverulent, and possessed many of the properties of albumen; but it differed from that principle, by the large proportion of phosphorus it contained in some unknown state of combination. The alcoholic solution was of a deep yellow colour, and deposited crystals of a sebaceous matter, and a portion of a yellow semi-fluid oil. On distilling off the alcohol, the

oil was obtained in a separate state. On cooling, it became nearly solid, and weighed 91 grains. The albuminous principle above mentioned, weighed 55.8 grains. Hence this yelk consisted of

	Grains.
Water . .	170.2
*Albumen ? .	55.8
Yellow Oil. .	91.0
	316.5

But I have reason to believe that the proportions of these ingredients differ a little in different eggs.

Experiments on the egg at the end of the first week of incubation, or about the 8th day.

At the end of the first week, it has been found, on an average, that on the supposition that the egg originally weighed 1000 grains, it has now lost about 50 grains, and the weights of the constituent principles of two eggs in their moist state, were as follow:

	No. 1.	No. 2.
	Grains.	Grains.
Unchanged albumen	282.8	247.1
Modified albumen	179.8	
Liq. amnii, membranes, blood-vessels, &c.	97.0	275.2
Animal	22.0	
Yelk	301.8	324.5
Shell and loss	167.1	153.2
	1000.0	1000.0

* This proportion of the albuminous principle does not differ much from that stated to exist in the yelk of the common fowl, by Mr. HATCHETT. Philos. Trans. vol. cvi. p. 308.

The consideration of the organization, &c. of the incubated egg, like that of its constituent principles, does not fall within my present design; yet, as some points connected with these subjects seem to be illustrated by the present inquiry, and as my experiments would be scarcely intelligible without them, I shall make a few brief remarks on the general phenomena presented by the different constituent principles of the egg at those periods at which it has been submitted to examination.

It has been remarked by many observers, that soon after the process of incubation has commenced, the yelk becomes more fluid than usual, and that as the liquor amnii increases, that portion of the albumen occupying the upper and larger end of the egg, begins to assume a peculiar appearance. In the present experiments (in which the egg was always previously boiled), the liquor amnii and portion of albumen in question, at the period now under consideration, exhibited somewhat the appearance of curds and whey. Nor did the analogy consist in mere appearance; for the curdy looking matter, which was of a yellow colour, and which I have termed *modified* albumen, resembled the curdy part of milk in its properties, so far as to contain intermixed with it an oily or butyraceous principle. A portion of this oily principle, on being separated and examined, was found to be soluble in alcohol, of a bright yellow colour: and in short, to possess all the properties of the yellow oil existing in the yelk. The yelk at this period, as before observed, has become more fluid, and appears larger and of a paler colour than natural. Haller, indeed, asserts, that it has not increased in weight, but the above table renders the reverse very probable. These appearances of the albumen and yelk have induced most

observers to believe that an interchange of principles takes place between them, while others seem to have mistaken the yellow modified albumen for the yelk itself. That an interchange of principles has taken place, at least under the above circumstances, there can be no doubt; yet the two are not indiscriminately mixed; for when the egg has been previously boiled, the yelk, though softer than natural, is nevertheless rendered of a firmer consistence than the modified albumen, and can thus be readily separated from it; there is, moreover, a distinct line of demarcation between them, arising, apparently, from the proper membrane of the yelk. Another argument in favour of the opinion of the intermixture of the albumen and yelk at this period, is derived from the following analyses of these constituent principles of the egg; from which it will be found, that the quantity of the saline matter is diminished in the albumen, and increased in the yelk. It is a singular and striking fact, however, that although the oily matter of the yelk has made its way to the albumen, very little of the phosphorus, which exists in such large quantities in the yelk, has been removed with it.

No. 1.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Unchanged albumen	.18	.27	.19	1.03	.18
Modified albumen, liquor amnii, animal membranes, &c.	.08	.38	.45	1.17	.12
Yelk. . . .	.09	4.03	.60	.80	.68
	.30	4.68	1.24	3.00	.98

No. 2.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Unchanged Albumen	.18	.18	.24	1.50	.12
Modified albumen, liquor amnii, animal membranes, &c.	.10	.25	.80	.70	.12
Yelk	.08	4.00	.56	.75	.67
	.36	4.43	1.10	2.95	.91

The following are the results of an analysis made two days later, or on the 10th day of incubation.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Unchanged Albumen	.27	.14	.24	1.13	.12
Modified albumen, liquor amnii, animal membranes, &c.	.08	.65	.68	1.86	.27
Yelk	.05	3.35	.30	.62	.66
	.40	4.14	1.22	3.11	1.05

At this period the proportion of phosphorus is somewhat diminished in the yelk, and increased in the animal and its appendages. The chlorine and alkaline principles seem also to have diminished in the yelk, and to have increased a little in the albuminous portion.

How the above interchange of principles takes place between the albumen and yelk, does not appear to be distinctly understood. MAITRE JEAN, LEVEILLE, and others, suppose that it takes place through the chalazæ; and LEVEILLE has even pretended to demonstrate the tubular structure of one of the chalazæ. This tubular structure has been denied by some writers, and particularly by Dr. MACARTNEY,* who even appears to doubt the fact of the intermixture in question. After what has been said, however, there cannot, I think, be much doubt of the circumstance; and future inquirers on this interesting, but difficult subject, will do well to turn their attention to it.

Experiments on the egg at the end of the second week, or about the 15th day of incubation.

At the end of the second week of incubation, an egg has lost upon an average about 130 grains, on the supposition that its original weight was 1000 grains, and the weights of the constituent principles of two eggs were as follow:

	Grains.	Grains.
Unchanged Albumen . .	175.5	208.0
Liquor amnii, membranes, &c.	278.5	218.2
Animal	70.0	89.1
Yelk	250.7	248.0
Shell and Loss . . .	280.3	236.7
	1000.0	1000.0

At this period the animal has attained a considerable size, while the albumen has become diminished in a corresponding

* Article INCUBATION, in REES's Encyclopædia.

degree. The albumen has also acquired a very firm consistence, especially when coagulated by heat. The liquor amnii has become more fluid, and the *modified* albumen, formerly mentioned, has very much diminished in quantity, or disappeared.* The yelk, which at the end of the first week seemed to have increased in bulk and fluidity, has now apparently acquired its original size and consistence. The following are the results of the analyses of the constituent principles of the above two eggs.

No. 1.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Unchanged albumen. -	.07	.22	.09	.78	.10
Liq. amnii, membranes, &c.	.06	.21	.71	.96	.08
Animal - - -	.06	.23	.09	.46	.27
Yelk - - -	.30	3.34	.16	.68	.69
	.49	4.00	1.05	2.88	1.14

No. 2.

Unchanged albumen.	.11	.19	.28	.97	.09
Liq. amnii, membranes, &c.	.03	.20	.70	1.07	.08
Animal - - -	.06	.24	.07	.44	.28
Yelk - - -	.20	3.30	.10	.42	.70
	.40	3.93	1.10	2.90	1.15

* About this time HARVEY, and other observers, have noticed the appearance of a curdy or coagulated substance in the œsophagus, crop, stomach, and intestines of the animal. Is this a portion of the *modified* albumen above mentioned?

An egg, analyzed two days later, or on the 17th day of incubation, gave the following results:

No. 3.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Liquor amnii, membranes, animal, &c. - -	.34	1.70	.68	2.40	1.10
Yelk - - -	.10	2.50	.80	.56	.75
	.44	4.20	.98	2.96	1.85

At this period ossification, which, according to HALLER and others, begins about the 7th day, has made some progress. The yelk has parted with some of its phosphorus, which appears in the other principles of the egg; this is particularly observable in No. 3. The quantity of earthy matter has also increased, particularly in No. 3.

Experiments on the egg at the end of the third week, or at the full period of incubation.

At this period an egg has lost upon an average about 160 grains, on the supposition that its original weight was 1000 grains; and the weights of the constituent principles of two eggs in their moist state and without boiling, were as follow:

	Grains.	Grains.
Residuum of albumen, membranes, &c.	29.5	38.1
Animal - - - - -	555.1	553.6
Yelk - - - - - -	167.7	151.3
Shell and loss - - - -	247.7	257.0
	1000.0	1000.0

At this period all the important changes of incubation are completed. The albumen has now disappeared, or is reduced to a few dried membranes and an earthy residuum (apparently consisting of the original earthy matter of the albumen which has remained unappropriated). The yelk is considerably reduced in size*, and is taken into the abdomen of the chick, while the animal has attained a weight nearly corresponding to the original weight of the albumen, added to that lost by the yelk, *minus* the total weight sustained by the egg during incubation. The alkaline matters and chlorine, which have been decreasing from the commencement of incubation, have now undergone farther diminution in quantity,† while *the earthy matters have increased in the most striking manner.* The other principles seem to have suffered very little change in quantity. The following are the results of the analyses of the above two eggs.

No. 1.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Resid. of albumen, membranes, &c.	.04	.12	.09	.28	.12
Animal - -	.44	3.02	.55	2.26	2.38
Yelk - - -	.04	1.06	.03	.06	1.26
	.52	4.20	.67	2.55	3.96

* This has been denied or doubted by some writers, especially Haller and Dr. Macartney.

† We have seen that in boiling an egg, a portion of its saline matter makes its way through the shell into the water. Do the saline matters, in the present instance, escape with the water lost during incubation? An argument in favour of the supposition is, that the loss is chiefly confined to those salts already existing in the egg.

No. 2.

	Sulphuric Acid.	Phospho-ric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Resid. of albumen, membranes, &c.	.03	.13	.09	.25	.12
Animal - -	.21	2.71	.68	2.12	2.60
Yelk - -	.02	1.23	.06	.03	1.10
	.26	4.07	.83	2.40	3.82

It may be proper to observe, that the above analyses are selected as the most perfect, from a variety of others made at each period, all of which confirm the results here given, though they differ, like those indeed given, in some subordinate particulars.*

These experiments, then, demonstrate, or render probable, the following circumstances.

1. That the relative weights of the constituent principles of different eggs vary very considerably.

2. That an egg loses about one-sixth of its weight during incubation, a quantity amounting to eight times as much as it loses in the same time under ordinary circumstances.

namely, the chlorine and alkaline matters. With respect to the use of these saline matters we know very little. Do they perform an office in the animal economy analogous to acid solutions in the galvanic battery?

* An interesting circumstance may be here mentioned, which I have never seen noticed by any writer on the present subject. At the end of the process of incubation, and for some time before, the animal is so situated in the egg, as, by its superior weight on one side, to cause the egg to assume such a position that the beak of the animal shall be uppermost, and consequently fully exposed to the air when it first makes its way through the shell.

3. That in the earlier stages of incubation, an interchange of principles takes place between the yelk and a portion of the albumen; that this interchange is confined on the part of the yelk to a little of its oily matter, which is found mixed with the above mentioned albumen; that this portion of albumen undergoes some remarkable changes, and is converted into a substance analogous in its appearance, as well as in some of its properties, to the curd of milk; and lastly, that a portion of the watery and saline portion of the albumen is found mixed with the yelk, which becomes thus apparently increased in size.

4. That as incubation proceeds, the saline and watery parts again quit the yelk, which is thus reduced to its original bulk; that in the last week of the process it undergoes still further diminution in weight, and loses the greater portion of its phosphorus, which is found in the animal converted into phosphoric acid, and in union with *lime*, constituting its *bony skeleton*; and lastly, that this lime does not originally exist in the recent egg, but is derived from some unknown source during the process of incubation.

These, and other interesting circumstances, arising out of the present inquiry, suggest some important hints respecting certain operations of the animal economy. They also serve to direct the attention of the microscopic inquirer to the investigation of points, which it is probably within his power to elucidate, but of which, at present, we are ignorant. This part of the subject, however, cannot be in abler hands than it is at present. To the distinguished physiologists, therefore, who have been recently engaged in the investigation, I willingly leave it, and shall conclude with a few remarks only

on the uses of the yelk, and the apparent generation of earthy matter.

Sir Everard Home and Mr. Hatchett have concluded, from their experiments, that the yelk is analogous to the milk of viviparous aimals, but more concentrated, and that its chief use is to afford a pabulum to the young animal during incubation.* This opinion, which is indeed as old as Aristotle,† is corroborated in a striking manner by the present inquiry. Mr. Hatchett has also made the important and curious remark, that in the ova of those tribes of animals, the embryos of which have bones, there is a portion of oily matter; and in those ova whose embryos consist entirely of soft parts, there is none. Hence it is concluded, that a certain portion of oil is necessary for the formation of bone. The present inquiry cannot be said to confirm, or invalidate, this remark, for although in the earlier stages of incubation, before ossification has commenced, a portion of the oil of the yelk is appropriated to the purposes of the economy of the animal, yet by far the greater portion of it remains; and some of it is even retained by the yelk till its final disappearance.‡ One great use of the yelk evidently is to fur-

* Philos. Trans. Vol. 106, p. 301, et seq.

† Ἡ μὲν οὖν ἀρχὴ τοῦ γινομένου ἐκ τοῦ λεκιθοῦ, ἡ δὲ τροφὴ διὰ τοῦ ὑμενώδους ἐκ τοῦ ἀχόρυ. Aristotelis de Animal. Hist. VI. 3. (Ed. Schneider.) Pliny makes the same remark. Ipsum animal ex albo liquore ovi corporatur. Cibus in luteo est. Hist. Nat. X. 53.

‡ I examined a chick on the 18th day after incubation. The yelk was now reduced to less than 2 grains, but it was of its original yellow colour, and of course contained oily matter. When burnt, it left traces of phosphate of lime. Dr. Macartney attempts to show that the yelk does not pass into the intestine through the ductus vitello intestinalis, but is taken up by absorption; and an argument he adduces in support of this opinion, is, that the earthy matter is left

nish the phosphorus, entering as phosphoric acid, into the skeleton of the animal; but that the earthy portion of the bones is derived from the transmutation of oil into lime cannot, perhaps, be safely asserted in the present state of the inquiry.

With respect to the earthy matter found in the skeleton of the chick when it quits the shell, I think I can venture to assert, after the most patient and attentive investigation, that it does *not pre-exist in the recent egg*; certainly not, at least in any known state. The only possible sources, therefore, whence it can be derived, are from the shell, or transmutation from other principles. Whether it be actually derived from the shell, cannot be determined by chemistry; because, as we have seen, the shells of different eggs differ so much, that the application of averages is out of the question; and we are of course precluded from ascertaining the exact quantity of lime any particular shell originally contains. There are, however, very strong reasons for believing that the earthy matter is not derived from the shell. In the first place, the membrana putaminis never becomes vascular, and seems analogous to the epidermis; hence the lime of the shell, which is exterior to this membrane, is generally considered by physiologists as *extra-vascular*;* it is therefore extremely difficult

behind in the yelk. In the present instance, however, the quantity of earthy matter was very minute: it had therefore disappeared, as well as the other principles of the yelk. When the chick is younger, the quantity of earthy matter is said to be much larger. HALLER asserts that the yelk disappears about the 16th day; and ARISTOTLE long ago remarked, that very little of it was left on the 10th day, after the chick had left the egg.

* See an Essay "on the connection between the vascular and extra-vascular parts of animals," by Sir A. CARLISLE. THOMSON's Annals, Vol. VI. p. 174.

to conceive how the earth in question can be introduced into the economy of the chick from this source, particularly during the last week of incubation, when a very large portion of the membranes are actually separated from the shell. Secondly, both the albumen and yelk contain, at the end of incubation, a considerable proportion of earthy matter (the yelk apparently more than it did originally); why is this not appropriated, in preference to that existing in the shell? In opposition to these arguments it will be doubtless stated, that the shell of the egg becomes brittle at the end of incubation, and appears to undergo, during that process, some other changes not at present understood. To which it may be answered, that this brittleness has been attributed to the separation of the membrana putaminis, and the exsiccation of the parts by so long an exposure to the heat necessary to the process of incubation; and in this manner all the *known* changes produced on the shell by incubation may perhaps be satisfactorily accounted for. Until, therefore, it be demonstrated that some other changes take place in the shell, I confess this argument does not seem to me to have much weight. I by no means wish however to be understood to assert, that the earth is *not* derived from the shell; because, in this case, the only alternative left me is to assert that it is formed by transmutation from other matter; an assertion, which I confess myself not bold enough to make in the present state of our knowledge, however strongly I may be inclined to believe that, within certain limits, this power is to be ranked among the capabilities of the vital energies.

XXIX. *On the Placenta.* By Sir Everard Home, *Bart. V. P. R. S.*

Read June 27, 1822.

Till the human ovum had been detected in the uterus, before it became attached, we could not have ascertained that the rudiments of the placenta were not formed at the same period with the chorion, and were, therefore, a part of the ovum belonging wholly to the female.

That fortunate discovery enables us now to determine that the placenta, and the changes in the chorion, which in some animals fit it to perform the functions of a placenta, depend as much upon the compound influence of the male and female, as the peculiarities of the embryo itself.

That the umbilical arteries had their origin from the branches of the aorta called iliacæ internæ; that these passed along the sides of the urinary bladder to the navel, where they composed the navel string, has been long known to anatomists; but in what manner the amnion was broken through, to allow these arteries to come at the chorion, has not even at this day been satisfactorily explained.

This must now be admitted to take place by the urinary bladder, on its first formation, bursting the amnion, in the same manner as the vesicle in the embryo of the bird bursts the membrane of the yelk, as is shown in the microscopical drawings annexed to a Paper I recently laid before the Society.

When it is considered that the office of the placenta is to supply the circulation of the blood in the embryo with ma-

terials for the growth of its different parts, and when we find so great a variety in the structure of placentas in different animals, it seems to follow that every genus of animals, in as much as it has a peculiar form, should have a peculiar placenta; and this, upon examination, is found to be the case.

The difference in the form of the placenta, which has hitherto had so little attention paid to it, is therefore to be considered as the means employed by nature to prevent the whole system respecting animals from being thrown into confusion, by preventing any two different genera from breeding together. We now see why this cannot take place, since a new form of placenta would be required intermediate between those of the two genera, for which there is no provision.

Upon the structure of the placenta or chorion must depend the period of utero-gestation. Where they are very vascular, it will be short, and where the reverse, very long. The human placenta is massy, thick, its arteries very large, and numerous; the utero-gestation is nine months. The placenta is wanting in the mare, there being only a very vascular chorion; the utero-gestation in that genus is eleven months. The utero-gestation of the elephant, according to Mr. CORSE, is twenty-two months; from which I am led to conclude there is no placenta.

The placenta would appear to be more or less in a perfect state, according to the care which is taken of the animal. The cow's utero-gestation at a mean is 284 days. The wild cow is stated to go 308, the longest period I believe respecting cows upon record.

This explains the latitude met with in utero-gestation, which is noticed in the Bulletin de Sciences, by the Philomatique Society in Paris, for the year 1797, by Mr. TESSIER.

In 160 cows, some calved in 241 days; 5 in 308; giving a latitude of 67 days.

In 108 mares, 8 foaled in 311 days; 1 in 394 days; giving a latitude of 83 days.

In 15 sows, 1 littered in 109 days; 1 in 123 days; giving a latitude of 14 days.

In 139 rabbits, 1 produced its young in 26 days; 9 in 33 days; giving a latitude of 7 days.

As the human species has the form of placenta best fitted for the supply of blood to the fœtus, there appears to be less latitude than in other animals, at least in civilized society, where the nourishment of the mother is an object of the first attention; it may be otherwise in women living in a savage state.

Where the female of one species of animals breeds from the male of another, the utero-gestation of whose species is different, there appears to be no approximation in the time the hybrid is brought forth; but the longest period of the two is the time of such utero-gestation.

The mare, when covered by an ass, goes 11 months, her usual period. The Earl of MORTON's mare, covered by the quagga, went 389 days 19 hours.

The she ass, when covered by a horse, goes 11 months, although ten is her usual period.

The direct cause of parturition has never been satisfactorily explained; and the great latitude there is in the utero-gestation of individuals of the same species, which has just been shown, makes it evident, that the cause must be something immediately connected with the complete formation of the fœtus. Upon this subject I am induced to hazard the following observations.

The lungs are the last parts of the embryo that are completely formed. As soon as this happens, the blood that circulates through the lungs must greatly diminish the supply of blood to the placenta; the consequence of which will be, that the small terminal arteries of the foetal portion will become contracted; which cannot happen to any great degree without producing more or less of a separation of the placenta and chorion, and this will be followed by the expulsion of the young. That very slight disturbance at this period brings on labour, is well known, from the number of children prematurely born, that are with difficulty kept alive till the circulation through the lungs becomes complete: they are called blue children.

I shall not carry these observations farther at present, and shall conclude them with a specimen of a new mode of classing animals, upon the principles laid down respecting the difference in structure of the placenta.

It will have the advantage, that the characters are fixed, and therefore the arrangement will never require to be changed: and there is a circumstance in its favour—it places the human species in an order separate from all inferior animals, the place undoubtedly assigned for mankind by the Almighty Creator.

Class I.

includes all animals in which the ovum becomes attached to the womb of the mother.

Ova with adherent chorions.

7 Orders.

1. Lobulated placenta. *Vide* Plates XLII. and XLIII.

One genus; one species; Man.

2. Subdivided placenta. *Vide* Plates XLIV. and XLV.
   One genus; all the Monkey tribe.
3. Belted placenta. *Vide* Plate XLVI.
   Two genera; first belt thick; Lion tribe.
   second belt thin; Dog tribe.
4. Placenta with many divisions. *Vide* Plate XLVII.
   One genus, with five divisions; Hare tribe.
   The others not yet known.
5. Cotyloid placenta.
   Genera 5.
   1. simple; Hedgehog.
   2. plane; Mole.
   3. thick; Bat.
   4. pedunculated; Rat.
   5. pediculated; Guinea-pig.
6. Numerous cotyledons.
   Genera 4.
   1. the terminal arteries, with lateral branches; Cow.
   2. the terminal arteries filiform; Deer.
   3. the terminal arteries villous; Sheep.
   4. the terminal arteries like shag; Goat.
7. Chorion without placenta.
   Genera 4.
   1. projecting plexuses; two species.
      Thick; Mare.
      Thin; Ass.
   2. stellated; Hog.
   3. vascular membrane; Camel.
   4. tufted; Whale.

EXPLANATION OF THE PLATES.

PLATE XLII.

The fœtal surface of the human placenta. The amnion turned a little inwards.

PLATE XLIII.

The uterine surface of the human placenta.

PLATE XLIV.

The fœtal surface of the placenta of the Monkey. The whole surface covered by the amnion.

PLATE XLV.

The uterine surface of the placenta of the Monkey.

PLATE XLVI.

Fig. 1. A fœtus of the Cat, inclosed in its membranes, to show the uterine surface of its circular placenta.

Fig. 2. The fœtal surface of the placenta of the Rabbit, divided into five lobes.

Fig. 3. The uterine surface of the same placenta.

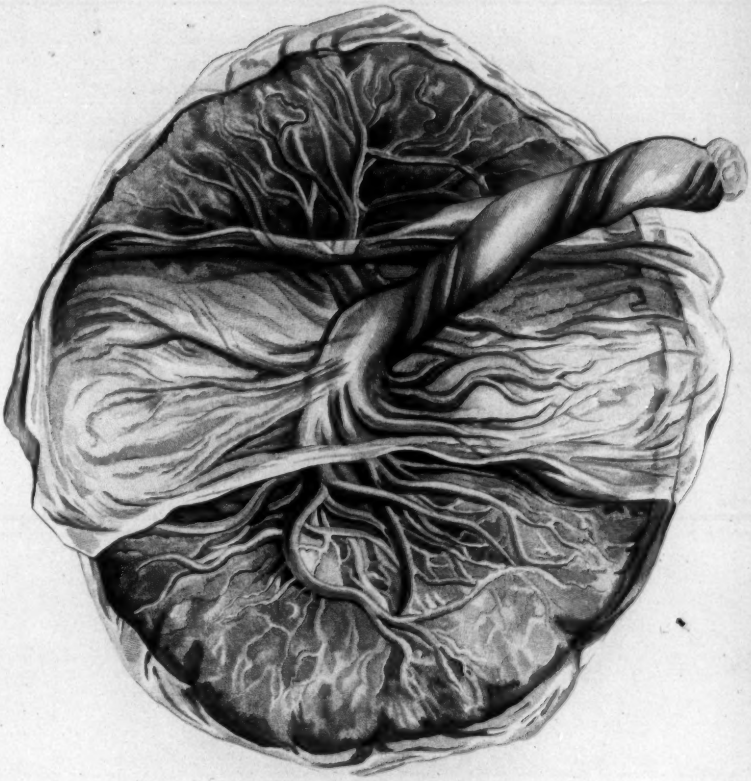
PLATE XLVII.

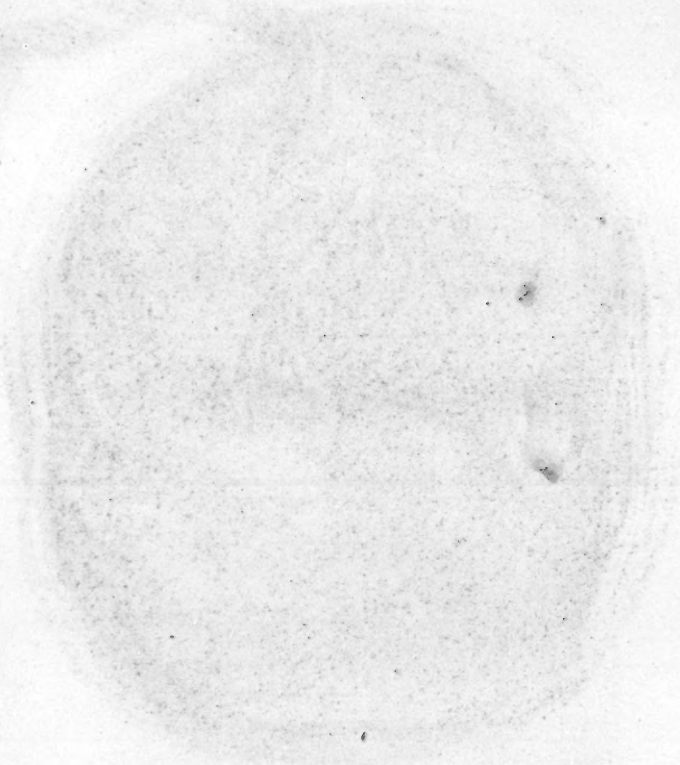
Shows the cotyloid placentæ.

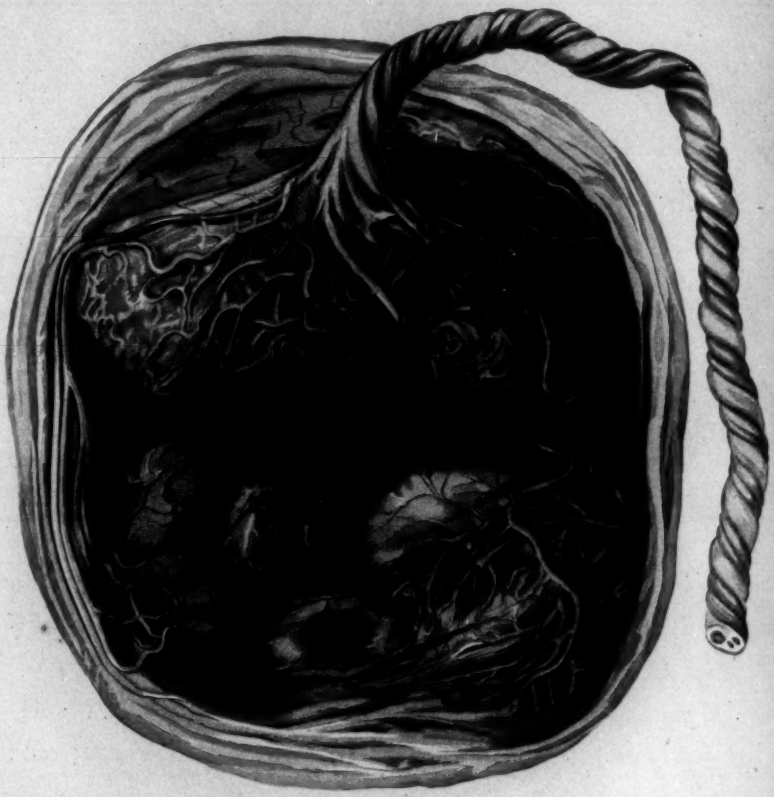
Fig. 1. Of the Hedge-hog.

Fig. 2. Of the Rat.

Fig. 3. Of the Guinea-pig.







W.m Clift del.t

J.s Basire sculp.t

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 1.

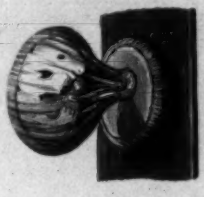
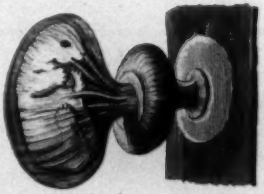


Fig. 2.

Fig. 3.



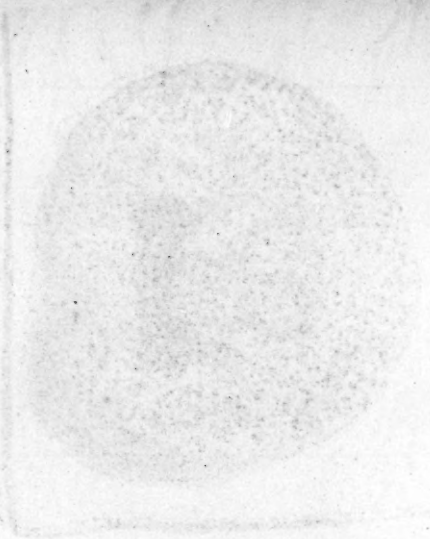


Fig. 1.

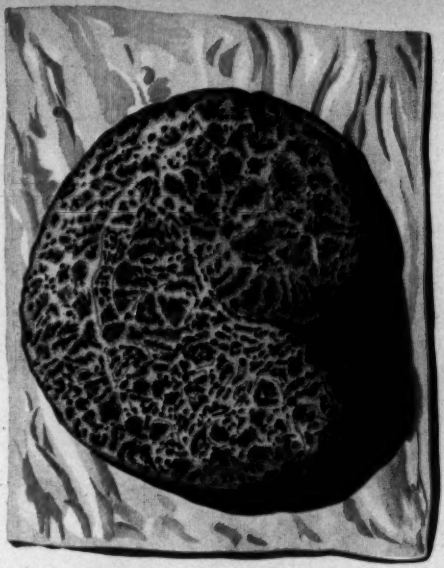


Fig. 3

Fig. 4.

Fig. 2.

Fig. 5.

PLATE XLVIII.

Fig. 1. The uterine portion of the cotyledon of the Cow.

Fig. 2. The uterine surface of the fœtal portion of the cotyledon.

Fig. 3. The terminating vessels of the fœtal portion of the cotyledon of the Deer.

Fig. 4. The terminating vessels of the fœtal portion of the cotyledon of the Sheep.

Fig. 5. The terminating vessels of the fœtal portion of the cotyledon of the Goat.

XXX. *Of the geographical situation of the Three Presidencies, Calcutta, Madras, and Bombay, in the East Indies.* By J. GOLDINGHAM, *Esq. F. R. S.*

Read June 27, 1822.

IN the present advanced state of knowledge it may be useless to dwell upon the importance to navigation, as well as to general geography, of correct information relative to the latitudes and longitudes of the principal places on the surface of our globe. The ease with which the situation of a place *on* the meridian is obtained, for general purposes, is well known, and the comparative difficulty of ascertaining the distance, east or west, *from* a given meridian is equally so, particularly where that meridian is a quarter of the globe distant, which is the case as relates to India. Having, however, one point correctly determined, the situations of others, at moderate distances from it, may be come at with greater facility; either by chronometers, by correspondent observations, or, where places are on the same continent, by actual survey.

One of the best methods of determining the position of a point, thus distant from the first meridian, is by eclipses of the satellites of Jupiter. Correspondent observations of eclipses of the sun, of the moon, or of occultations, happen but seldom, and the method by the moon's transit requires, that the position of that luminary should be correctly set down in the Tables; or, in the case of correspondent transits, that the instruments at both places should be most accurately

placed in the meridian, and the transits taken with the least possible error of observation; as only a very small error in the Tables, or in the observed place of the moon, may produce a considerable one in the result. But eclipses of the satellites of Jupiter occur often, and correspondent ones with those taken at Greenwich, are not *very* unfrequent, even in this distant part of the globe. The observations taken at Greenwich also show the difference or error of the Tables, and consequently, the error of the longitude deduced from them. Errors also which may arise from a difference in the powers of the telescopes, and in the eyes of observers, as well as from a general difference in the state of the atmosphere, may be counterbalanced by taking a series of these eclipses, consisting of immersions as well as emersions.

I shall, therefore, for the present at least, as regards the longitude of Madras, draw a conclusion from these eclipses; a very long catalogue of which has been taken at the Madras Observatory. So numerous, indeed, are these observations, that the longitude of Madras, which I may give at a future time, by other methods, may perhaps be considered more as corroborating *that* now deduced, than as furnishing information for correcting it.

We may now, however, notice a result obtained from lunar observations. Of these, about 800 have been taken at various times since the year 1787, with different sextants; and reduced to the Observatory, give its longitude $2'\ 55,''5$ more than by the satellites. This will furnish us with a correction for numerous observations of this description taken at Bombay, when we come to treat of the longitude of that place.

The first set of the following eclipses is composed of ob-

servations taken at different places in India; the differences of meridians between which and the Observatory are correctly known, either from correspondent eclipses, chronometers, or by survey. These having been taken with different telescopes, and by different observers, and also at a distance from the Observatory, may be considered as less valuable than those observed there, with the same description of telescope, and under every favourable circumstance.

The second result is from eclipses taken at Madras, with different telescopes, at two or three different points, and reduced to the Observatory. These may also be considered of less value than the third result, which is drawn from eclipses taken at the Observatory with the same description of telescope, and under favourable circumstances. I have, therefore, in drawing the conclusion, considered a mean of the first and second results as about equal in value to the third, and have combined them accordingly. The fourth result is from correspondent eclipses, which I considered of equal value with the other three results. This relates to the first series of eclipses from 1787 to 1801.

In the second series, containing eclipses taken between the years 1803 and 1816, the first result was deduced from observations taken at the Observatory with the same description of telescope, and is therefore of equal value with the mean of the two first results, and also with the third of the other series; and the results have been combined accordingly. The second result of the second series is obtained from correspondent eclipses; and, like the former, by observations of this description, is considered of equal value with the mean of all the results found by correcting the Tables.* The differ-

* In drawing the conclusion, I have combined *all* the correspondent eclipses.

ences, applied to the longitude, found by the Tables in the Ephemeris were obtained by eclipses taken at Greenwich, as near the time as possible that each eclipse was observed at Madras.

The following are the observations and results. It may be proper to state, that some additional observations were at first included in these Tables; that a mean was taken; and, when any longitude differed more than 30 seconds from the mean, it was rejected; and it is only these eclipses which were within 30 seconds of the general mean that are here included. The observations being so numerous, enabled me to make this selection. The general result in both cases is however very nearly the same, as is commonly the case; there being found as many rejected observations giving a longitude too great as too little.*

* In finding the difference of the Tables, reference has been made to the circumstance under which the Greenwich observation *nearest* the time was taken, and its value in consequence; as well as to other observations taken *about* the time.

Longitude of the Madras Observatory by the Eclipses of the Satellites of Jupiter, from 1787 *to* 1801, *corrected for the difference of the Tables from the Observations taken at Greenwich at or about the time of each Eclipse.*

Day.	Place.	Satellites.	Apparent Time. Observed at Madras.	Apparent Time. Per Ephemeris.	Longitude in Time.	Difference of the Tables.	Corrected Longitude.	Difference of Longitude to the Observatory.	Longitude of the Observatory.
1787.			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.	m. s.	h. m. s.
Feb. 9	Calcutta.	1 E	8 33 13	2 39 36	5 53 37	+ 14	5 53 51	32 25,2	5 21 25,8
16	——	1 E	10 29 26	4 35 34	5 53 52	+ 14	5 54 6	32 5,2	5 21 40,8
22	——	2 E	8 26 48	2 33 48	5 53 00	+ 1,20	5 54 20	32 28	5 21 54,8
25	——	1 E	6 55 22	1 1 23	5 53 59	+ 14	5 54 13	32 28	5 21 47,8
1789.									
Dec. 17	Coringa.	1 I	17 6 58,5	11 38 41	5 28 17,5	+ 27	5 28 44,5	8 19,7	5 20 24,8
19	——	1 I	11 34 23,2	6 6 9	5 28 14,2	+ 28	5 28 42,2	8 19,7	5 20 22,5
29	——	2 I	13 28 53	8 0 17	5 28 36	+ 12	5 28 48	8 19,7	5 20 28,3
1790.									
Jan. 2	——	1 I	15 14 41,4	9 46 9	5 28 32,4	+ 32	5 29 4,4	8 19,7	5 20 44,7
23	Masulipatam.	2 I	10 15 49	4 51 46	5 24 3	+ 47	5 21 50	3 38,3	5 21 10,3
Feb. 1	——	1 I	17 4 4	11 40 3	5 24 1	+ 39	5 24 50	3 38,3	5 20 0,3
26	——	1 E	13 59 46	8 35 24	5 24 22	+ 48	5 25 10	3 38,3	5 21 30,3
1791.									
Jan. 5	Bombay.	1 I	17 30 54,3	12 40 6	4 50 48,3	+ 30	5 51 18,3	29 38,4	5 20 56,7
7	——	1 I	11 58 2	7 7 42	4 50 20	+ 32	4 50 52	29 38,4	5 20 30,4
Feb. 20	——	1 I	17 41 35	12 50 52	4 50 43	+ 24	4 51 7	29 38,4	5 20 45,4
22	——	1 I	12 10 3	7 19 32	4 50 31	+ 26	4 50 57	29 38,4	5 20 35,4
March 1	——	1 I	14 5 10,5	9 14 43	4 50 27,5	+ 55	4 51 2,5	29 38,4	5 20 40,9
8	——	1 I	16 0 31,5	11 10 21	4 50 10,5	+ 30	4 50 40,5	29 38,4	5 20 18,9
24	——	1 E	16 35 26	11 43 39	4 51 47	+ 19	4 52 6	29 38,4	5 21 44,4
April 4	——	1 E	7 29 30	2 38 16	4 51 14	+ 39	4 51 53	29 38,4	5 21 31,4
11	——	1 E	9 25 39	4 34 87	4 21 2	+ 34	4 51 36	29 38,4	5 21 14,4
18	——	1 E	11 22 31	6 30 47	4 51 44	+36, 3	4 52 20,3	29 38,4	5 21 58,7
25	——	1 E	13 17 51,2	8 26 45	4 51 6,2	+ 43	4 51 49,2	29 38,4	5 21 27,6
27	——	1 E	7 46 41,2	2 55 41	4 51 0,2	+ 43	4 51 43,2	29 38,4	5 21 21,6
May 27	——	2 E	9 54 2,8	5 3 18	4 50 44,8	+ 51	4 51 35,8	29 38,4	5 21 14,6
1789.									
Jan. 29	Tranquebar.	2 E	14 21 10	9 1 26	5 19 44	— 12	5 19 32	+1 34	5 21 6
31	——	1 E	10 40 54	5 22 5	5 18 49	+ 1,10	5 19 59	+1 34	5 21 33
Feb. 14	——	1 E	14 29 56	9 10 25	5 19 31	+12, 5	5 19 43,5	1 34	5 21 17,5
23	——	1 E	10 54 17	5 34 36	5 19 41	+12, 5	5 19 53,5	1 34	5 21 27,5
May 28	——	1 E	6 33 4	1 13 20	5 19 44	+ 20	5 20 4	+1 34	5 21 38
1790.									
Jan. 23	——	2 I	10 10 12	4 51 46	5 18 26	+ 1, 6	5 19 32	1 34	5 21 6
25	——	1 I	15 5 52	9 47 23	5 18 29	+38,	5 19 7	1 34	5 20 41
30	——	2 I	12 43 29	7 25 39	5 17 50	+ 1,6	5 18 56	1 34	5 20 30
1787.									
Nov. 19	Madras.	2 I	8 18 54	2 58 8	5 20 46	+ 1, 7	5 21 53	1 2	5 21 51,8
Dec. 21	——	2 E	10 35 57	5 14 3	5 21 54	— 27	5 21 27	1 2	5 21 25,8
28	——	2 E	13 10 10	7 48 32	5 21 38	— 27	5 21 11	1 2	5 21 9,8
1788.									
Jan. 27	——	1 E	13 1 14	7 41 6	5 20 8	+ 50	5 20 58	1 2	5 20 56,8
Feb. 12	——	1 E	11 19 3	5 58 43	5 20 20	+ 50	5 21 10	1 2	5 21 8,8
23	——	2 E	10 1 56	4 42 0	5 19 56	+ 1,14	5 21 10	1 2	5 21 8,8
March 22	——	1 E	10 1 47,8	4 40 53	5 20 54,8	+ 12	5 21 6,8	1 2	5 21 5,6
31	——	1 E	6 28 44,2	1 7 54	5 20 50,2	+ 12	5 21 2,2	1 2	5 21 1
April 23	——	1 E	6 49 43,5	1 28 48	5 20 55,5	— 3	5 20 52,5	1 2	5 20 51,3

Day.	Place.	Satellites.	Apparent Time. Observed at Madras.	Apparent Time. Per Ephemeris.	Longitude in Time.	Difference of the Tables.	Corrected Longitude.	Difference of Longitude to the Observatory.	Longitude of the Observatory.
1788.			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.	s.	h. m. s.
Nov. 5	Madras.	1 I	15 44 47,1	10 24 20	5 20 27,1	+ 52	5 21 19,1	5,4	5 21 15,7
14	——	1 I	12 5 39,3	6 45 9	5 20 30,3	+ 52	5 21 22,3	5,4	5 21 16,9
28	——	1 I	15 48 47,6	10 28 10	5 20 37,6	+ 1, 0	5 21 37,6	5,4	5 21 32,2
30	——	1 I	10 16 37,8	4 55 52	5 20 45,8	+ 1, 0	5 21 45,8	5,4	5 21 40,40
1789.									
April 3	——	1 E	9 37 3	4 16 27	5 20 36	+ 27	5 21 3	1	5 21 2
26	——	1 E	9 56 17,6	4 35 36	5 20 41,6	+ 20	5 21 1,6	1,2	5 21 0,4
1790.									
Jan. 25	——	1 I	15 7 51,7	9 47 23	5 20 28,7	+ 38	5 21 6,7	9,4	5 20 57,3
30	——	2 I	12 46 18,1	7 25 39	5 20 39,1	+ 1, 6	5 21 45,1	9,4	5 21 35,7
Feb. 3	——	1 I	11 28 48,9	6 8 19	5 20 29,9	+ 40	5 21 9,9	1,2	5 21 8,7
10	——	1 I	13 22 13,3	8 1 58	5 20 15,3	+ 40	5 20 55,3	1,2	5 20 54,1
26	——	1 E	13 55 29	8 35 24	5 20 5	+ 48	5 20 53	1,2	5 20 51,8
28	——	1 E	8 24 25,2	3 4 17	5 20 8,2	+ 48	5 20 56,2	1,2	5 20 55
March 14	——	1 E	12 16 39,3	6 56 19	5 20 20,3	+ 32	5 20 52,3	1,2	5 20 51,1
21	——	1 E	14 13 00	8 52 45	5 20 15	+ 52	5 21 7	1,2	5 21 5,8
April 6	——	1 E	12 35 42,8	7 15 20	5 20 22,8	+ 42	5 21 10,8	1,2	5 21 9,6
8	——	1 E	7 4 30,9	1 44 27	5 20 3,9	+ 48	5 20 51,9	1,2	5 20 50,7
15	——	1 E	9 0 55,49	3 40 50	5 20 5,4	+ 49	5 20 54,4	1,2	5 20 53,2
22	——	1 E	10 57 13,4	5 36 58	5 20 15,4	+ 49	5 21 4,4	1,2	5 21 3,2
1792.									
March 19	——	1 I	15 46 24,3	10 25 55	5 20 29,3	+ 54	5 21 23,3	0,2	5 21 23,5
21	——	1 I	10 15 37	4 54 57	5 20 40	+ 54	5 21 34	0,2	5 21 34,2
28	——	1 I	12 11 21,2	6 51 7	5 20 14,2	+ 48	5 21 2,2	+ 0,2	5 21 2,4
May 13	——	1 E	14 52 5,5	9 31 40	5 20 25,5	+ 30	5 20 55,5	+ 0,2	5 20 55,7

The Coringa, Masulipatam, and Tranquebar observations were taken by the late Mr. TOPPING: the Calcutta observations also by the late Mr. TOPPING: the Bombay observations by myself.

At the Madras Observatory.

Day.	Satellites.	Apparent Time. Observed at Madras.	Apparent Time. Per Ephemeris.	Longitude in Time.	Difference of the Tables.	Corrected Longitude.
1793.		h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
March 24	1 I	13 7 11	7 46 44	5 20 27	+ 24	5 20 51
31	1 I	15 2 57	9 42 32	5 20 25	+ 24	5 20 49
April 7	1 I	16 58 46,4	11 38 19	5 20 27,4	+ 50	5 21 17,4
9	1 I	11 27 22,9	6 7 16	5 26 6,9	+ 50	5 20 56,9
16	1 I	13 3 29,7	8 2 58	5 20 31,7	+ 50	5 21 21,7
May 1	2 I	13 12 46,2	7 53 9	5 19 37,2	+1 34	5 21 11,2

In the Madras observations which follow, sometimes three observers have taken the eclipse, sometimes two; but all the telescopes have the same power, and are exactly of the same construction, having been made by DOLLOND at one and the same time.

The two assistants at the Observatory are Bramins: the head assistant is named SENVASSACHARY, and the second VERDACHARY.

Eclipses from 1794 *to* 1801, *with the same description of Telescope.*

Day.	Place.	Satellites.	Apparent Time. Observed at Madras.	Apparent Time. Per Ephemeris	Longitude in time.	Difference of the Tables.	Longitude of the Observatory.
1794.	Madras		h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
May 5	Observatory.	1 I	14 38 47	9 18 53	5 19 54	+ 48	5 20 42
12	——	1 I	16 33 14,3	11 13 2	5 20 12,3	+ 1,14	5 21 26,3
21	——	1 I	12 55 24,5	7 35 11	5 20 13,5	+ 1,14	5 21 27,5
28	——	1 I	14 48 46	9 28 37	5 20 19	+ 1,16	5 21 25
30	——	1 I	9 16 39,6	3 56 54	5 19 45,6	+ 1,16	5 21 16
June 4	——	1 I	16 41 41,3	11 21 44	5 19 57,3	+ 1,18	5 21 15,3
6	——	1 I	11 9 53	5 49 59	5 19 54	+ 1,18	5 21 12
10	——	2 I	16 52 37	11 32 26	5 20 11	+ 1, 2	5 21 13
1795.							
Sept. 4	——	1 E	9 57 44,5	4 37 10	5 20 34,5	+ 17	5 20 51,5
11	——	1 E	11 55 37,5	6 35 11	5 20 26,5	+23, 6	5 20 50,1
1796.							
July 22	——	1 I	10 30 14,9	5 9 28	5 20 46,9	+20, 4	5 21 7,3
29	——	1 I	12 24 15	7 3 33	5 20 42	+ 20	5 21 2
Sept. 13	——	1 E	15 14 3,6	9 53 8	5 20 55,6	+10, 2	5 21 5,8
1797.							
Oct. 18	——	1 E	15 27 59,5	10 7 7	5 20 52,5	+39, 9	5 21 32,4
20	——	1 E	9 56 39,3	4 36 9	5 20 30,3	+ 40	5 21 10,3
Nov. 3	——	1 E	13 47 55,7	8 27 20	5 20 35,7	+37, 7	5 21 13,4
5	——	1 E	8 16 26,7	2 56 6	5 20 20,7	+41, 4	5 21 2,1
12	——	1 E	10 11 14,3	4 50 49	5 20 25,3	+49, 2	5 21 14,5
1798.							
Jan. 29	——	1 E	6 56 37,3	1 35 51	5 20 46,3	+14, 6	5 21 0,9
Feb. 21	——	1 E	7 13 4,8	1 52 21	5 20 43,8	+ 8, 1	5 20 51,9
Oct. 7	——	1 I	13 1 48	7 40 24	5 21 24	16	5 21 8
16	——	1 I	9 26 12,3	4 4 53	5 21 19,3	12, 5	5 21 6,8
——	——	2 I	11 29 32	6 8 50	5 20 42	+ 3, 4	5 20 45,4
23	——	1 I	11 21 24,2	6 0 2	5 21 22,2	— 15	5 21 7,2
30	——	1 I	13 15 57,8	7 54 41	5 21 16,8	— 17	5 20 59,8
Nov. 17	——	1 E	8 7 14	2 46 45	5 20 29	+ 35	5 21 4
——	——	2 E	13 45 17,8	8 22 26	5 22 51,8	1,18	5 21 33,8
Dec. 12	——	2 E	10 47 12,5	5 25 34	5 21 38,5	—20, 9	5 21 17,6
1799.							
Jan. 16	——	1 E	12 1 19,6	6 40 36	5 20 43,6	+ 36	5 21 19,6
18	——	1 E	6 29 15,2	1 8 56	5 20 19,2	+ 35	5 20 54,2
25	——	1 E	8 23 10,5	3 2 42	5 20 28,5	+ 30	5 20 58,5
Oct. 3	——	1 I	14 34 57,6	9 14 1	5 20 56,6	+ 8, 2	5 21 4,8
10	——	1 I	16 30 18,7	11 9 26	5 20 52,7	— 0, 7	5 20 52
Sept. 15	——	2 I	13 32 42,6	8 14 34	5 21 8,6	+ 21	5 21 9,6
1800.							
Feb. 6	——	1 E	8 4 35,5	2 44 8	5 20 27,5	+ 37	5 21 4,5
13	——	1 E	10 0 4,8	4 39 12	5 20 52,8	+ 30	5 21 22,8
March 24	——	1 E	8 42 25	3 21 38	5 20 47	0, 0	5 20 47
Nov. 23	——	1 I	12 37 19,2	7 16 37	5 20 42,2	+30, 6	5 21 12,8

Correspondent Eclipses of the Satellites of Jupiter, from the year 1787 to 1800.

Day.	Place.	Satellites.	Apparent Time. Observed Place.	Apparent Time. At Greenwich.	Longitude in Time.	Difference of Longitude to the Observatory.	Longitude of the Observatory.
1787.			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
Dec. 21	Madras.	2 E	10 35 57	5 14 30	5 21 27	1,2	5 21 25,8
1789.							
Dec. 19	Coringa	1 I	17 6 58,5	11 38 14	5 28 44,5	8 21,7	5 20 22,8
1790.							
Jan. 25	Madras.	1 I	15 7 51,7	9 46 45	5 21 6,7	1,2	5 21 5,5
Feb. 26	Masulipatam.	1 E	13 59 46	8 34 36	5 25 10	3 39,7	5 21 30,3
March 3	Madras.	2 E	15 20 25,7	9 58 41	5 21 44,7	1,2	5 21 43,5
8	——	3 E	12 27 45,5	7 6 39	5 21 6,5	1,2	5 21 5,3
21	——	1 E	14 13 00	8 51 53	5 21 7	1,2	5 21 5,8
1791.							
March 1	Bombay.	1 I	14 5 10,5	9 14 8	4 51 2,5	+29 38,3	5 20 40,8
——	——	3 I	15 22 12	10 31 15	4 50 57	29 38,3	5 20 35,3
24	——	1 E	16 35 26	11 43 20	4 52 6	29 38,3	5 21 44,3
April 25	——	1 E	13 17 51,2	8 26 2	4 51 49,2	29 38,3	5 21 27,5
1792.							
March 19	Madras.	1 I	15 46 24,3	10 25 19	5 21 5,3		5 21 5,3
April 11	——	1 I	16 3 35	10 42 55	5 21 00		5 21 00
1793.							
May 8	Masulipatam.	2 I	15 50 11,4	10 25 36	5 20 35,4	3 39,7	5 20 55,7
1794.							
June 10	Madras Observatory.	2 I	16 52 37	11 31 24			5 21 13
1796.							
Sept. 13.	——	1 E	15 14 3,6	9 52 57,8			5 21 5,7
1797.							
Oct. 18	——	1 E	15 27 59,5	10 6 25			5 21 34,5
1798.							
Nov. 15	——	1 E	13 39 55,5	8 17 53,1			5 22 2,4
Dec. 12	——	2 E	10 47 12,5	5 25 54,8			5 21 17,7
1799.							
Jan. 16	——	1 E	12 1 19,6	6 40 02			5 21 19,4
Oct. 10	——	1 I	16 30 18,7	11 9 26,7			5 20 52

RESULTS

By the First and Second Satellites, observed at different places in India, but reduced to the Madras Observatory. Longitude by

Immersions.	Emersions.
h. m. s.	h. m. s.
5 20 24, 8	5 21 25, 8
20 22, 5	21 40, 8
20 28, 3	21 54, 8
20 24, 7	21 47, 8
21 11, 7	21 31, 7
21 1, 7	21 44, 4
20 56, 7	21 31, 4
20 30, 4	21 14, 4
20 45, 4	21 58, 7
20 35, 4	21 27, 6
20 40, 9	21 21, 6
20 18, 9	21 14, 2
21 6, 0	21 6, 0
20 41, 0	21 33, 0
20 30, 0	21 17, 5
	21 27, 5
5 20 41,23	21 38, 0
	21 31,48
	20 41,23
Mean	5 21 6,35 E

RESULTS

First and Second Satellites observed at Madras. Longitude by

Immersions.	Emersions.
h. m. s.	h. m. s.
5 21 51, 8	5 21 25, 8
21 13, 7	21 9, 8
21 16, 9	20 56, 8
21 32, 2	21 8, 8
21 40, 4	21 8, 8
20 57, 3	21 5, 6
21 35, 7	21 1, 0
21 8, 7	20 51, 3
20 54, 1	21 2, 0
21 23, 5	20 0, 4
21 34, 2	20 51, 8
21 2, 4	20 55, 0
20 51, 0	20 51, 1
20 49, 0	20 5, 8
21 17, 4	21 9, 6
20 56, 9	20 50, 7
20 21, 7	20 53, 2
21 11, 2	21 3, 2
	20 55, 7
5 21 15,45	5 21 1,39
	21 15,45
Mean	5 21 8,42

RESULTS

By Eclipses from 1794 *to* 1801. *First and Second Satellites observed at Madras, with the same Telescope. Longitude by*

Immersions.	Emersions.
h. m. s.	h. m. s.
5 20 42, 0	5 20 51, 5
21 26, 3	20 50, 1
21 27, 5	21 5, 8
21 25, 0	21 32, 4
21 1, 6	21 10, 3
21 15, 3	21 13, 4
21 12, 0	21 2, 1
21 13, 0	21 14, 5
21 7, 3	21 0, 9
21 2, 0	20 51, 9
21 8, 0	21 4, 0
21 6, 8	21 33, 8
20 45, 4	21 17, 6
21 7, 2	21 19, 6
20 59, 8	20 54, 2
21 29, 6	20 58, 5
21 4, 8	21 4, 5
20 52, 0	21 22, 8
21 12, 8	20 47, 0
5 21 8,34	5 21 7, 1
	21 8,34
Mean	5 21 7,72

RESULTS.

Correspondent Eclipses from 1787 *to* 1800. *First, Second, and Third Satellites. Longitude by*

Immersions.	Emersions.
h. m. s.	h. m. s.
5 20 22, 8	5 21 25, 8
21 5, 5	21 30, 3
20 40, 8	21 43, 5
20 35, 3	21 5, 3
21 5, 3	21 5, 8
21 0, 0	21 44, 3
20 55, 7	21 27, 5
21 13, 0	21 5, 8
21 52, 0	21 34, 5
	22 2, 4
	21 17, 7
	21 19, 4

Longitude of the Observatory by correcting the Tables, from 1803 *to* 1815.

Date.	Satellites.	Immersions or Emersions.	Mean Time by the Nautical Almanac.	Mean Time observed at Madras.	Longitude of Madras by Tables.	Difference of the Tables.	Longitude of Madras.
1803.			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
Feb. 1	1	Im.	6 1 52	11 23 14,53	5 21 22,43	o 18,0	5 21 4,4
18	2	Im.	8 5 34	13 27 1,39	5 21 27,39	o 28,0	5 20 59,4
March 15	2	Im.	5 5 33	10 26 35,37	5 21 2,37	o 28,0	5 20 34,4
April 2	2	Em.	2 11 22	7 32 18,48	5 20 56,48	+o 24,0	5 21 20,5
20	1	Em.	5 6 25	10 27 32,30	5 21 7,30	—o 21,0	5 20 46,3
27	1	Em.	7 0 45	12 21 46,85	5 21 1,66	—o 21,0	5 20 40,6
May 13	1	Em.	5 18 17	10 39 19,66	5 21 2,66		
20	1	Em.	7 12 47	12 33 55,26	5 21 8,26		
July 7	1	Em.	2 6 1	7 27 20,01	5 21 19,01	+o 6,0	5 21 25,0
1804.							
Jan. 12	1	Im.	8 44 19	14 5 7,60	5 20 48,60		
26	1	Im.	12 31 8	17 15 10,15	5 21 2,15		5 21 6,7
April 22	1	Em.	7 52 54	13 14 13,19	5 21 19,19	+o 4,5	5 21 24,7
24	1	Em.	2 21 23	7 42 42,67	5 21 19,67		5 21 24,2
May 1	1	Em.	4 15 41	9 36 40,83	5 20 59,33		5 21 4,3
4	2	Em.	2 45 28	8 6 51,90	5 21 23,90	+o 9,5	5 21 33,4
8	1	Em.	6 10 3	11 30 41,90	5 20 38,90		5 20 48,4
1805.							
March 10	1	Im.	8 15 31	13 36 34,63	5 21 3,63		
23	2	Im.	10 33 21	15 55 11,66	5 21 50,66		
24	1	Im.	12 2 28	17 23 25,33	5 20 57,33		
26	1	Im.	6 30 54	11 51 50,60	5 20 56,60		
April 2	1	Im.	8 24 32	13 45 25,56	5 20 53,56		
18	1	Im.	6 40 25	12 1 18,63	5 20 53,63		
25	1	Im.	8 34 21	13 55 11,33	5 20 50,33		
May 4	1	Im.	4 57 0	10 18 17,34	5 21 17,34		
July 21	1	Em.	4 7 35	9 28 4,35	5 20 29,35		
28	1	Em.	6 2 42	11 23 24,26	5 20 42,26		
1806.							
Feb. 18	1	Im.	11 6 43	16 27 34,86	5 20 51,86		
April 14	1	Im.	7 44 31	13 5 0,71	5 20 29,71		
18	2	Im.	9 0 9	14 21 47,09	5 21 38,09		
21	1	Im.	9 37 57	14 58 50,76	5 20 53,76		
May 14	1	Im.	9 47 11	15 7 53,87	5 20 42,87	+o 34,0	5 21 16,9
27	2	Im.	11 6 52	16 28 55,43	5 22 3,43		
Sept. 4	2	Em.	1 46 52	7 7 43,26	5 20 51,26	+o 12,0	5 21 3,2
Oct. 6	2	Em.	1 28 30	6 49 16,34	5 20 46,34	—o 22,0	5 21 8,3
1807.							
May 3	1	Im.	9 11 7	14 32 19,06	5 21 12,06		
10	1	Im.	11 4 31	16 25 40,30	5 21 0,30		
26	1	Im.	9 19 47	14 41 3,23	5 21 16,23		
June 2	1	Im.	11 13 22	16 34 32,90	5 21 10,90		
Aug. 11	2	Em.	6 53 31	12 14 41,88	5 21 10,88	+o 15,5	5 21 26,4
29	2	Em.	1 21 19	6 42 12, 8	5 20 53, 8		5 21 9,3
30	1	Em.	1 20 20	6 41 8, 6	5 20 48, 6	+o 15,5	5 21 4,1
Oct. 6	1	Em.	5 27 36	10 48 26, 1	5 20 50, 1	+o 8,0	5 20 58,1
22	1	Em.	3 48 10	9 9 2,	5 20 50, 0	—o 3,2	5 20 48,8
Nov. 7	1	Em.	2 8 52	7 29 40,	5 20 47, 0	+o 1,4	5 20 48,4
1808.							
May 21	1	Im.	10 53 36	16 14 40,18	5 21 4,18		
June 13	1	Im.	11 2 31	16 23 43,57	5 21 12,57		
22	2	Im.	12 5 34	17 26 29,67	5 20 55,67		
29	1	Im.	9 18 9	14 39 22,54	5 21 13,54		

Date.	Satellite.	Immersions or Emersions.	Mean Time by the Nautical Almanac.	Mean Time observed at Madras.	Longitude of Madras by Tables.	Difference of the Tables.	Longitude of Madras.
			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
1808.							
Sept. 17	1	Em.	2 57 48	8 18 15,16	5 20 27,16	+0 24,5	5 20 51,7
24							
Oct. 26	1	Em.	1 32 53	6 54 0,83	5 21 7,83	—0 12,0	5 20 55,8
Nov. 9	1	Em.	5 24 37	10 45 39,37	5 21 2,37	. . .	5 21 2,4
1809							
Aug. 26	1	Im.	9 38 18	14 59 22,31	5 21 4,31	—0 19,1	5 20 45,2
Oct. 22	1	Em.	3 1 58	8 23 1,32	5 21 3,32	. . .	5 21 3,3
Nov. 5	1	Em.	6 52 11	12 13 14,46	5 21 3,46	+0 2,0	5 21 5,5
7	1	Em.	1 20 58	6 42 2,04	5 21 4,04	+0 3,5	5 21 7,5
14	1	Em.	3 16 19	8 37 26,98	5 21 7,98	+0 4,8	5 21 12,8
Dec. 4	2	Em.	5 8 22	10 28 37,28	5 20 15,28	+0 50,3	5 21 5,5
7	1	Em.	3 32 10	8 53 9,75	5 20 59,75	+0 2,0	5 20 59,9
23	1	Em.	1 52 51	7 14 1,27	5 21 10,27	+0 1,5	5 21 11,8
29	2	Em.	2 15 46	7 36 15,20	5 20 29,20		
30	1	Em.	3 48 50	9 10 4,25	5 21 14,25	+0 1,0	5 21 15,2
1810.							
Jan. 30	2	Em.	1 58 10	7 18 42,93	5 20 32,93		
March 3	2	Em.	1 39 14	7 0 5,25	5 20 51,25	+0 12,4	5 21 3,7
June 10	2	Im.	11 44 27	17 5 20,07	5 20 53,07		
July 30	1	Im.	11 13 50	16 34 48,95	5 20 58,95		
Oct. 9	1	Im.	6 9 57	11 30 55,03	5 20 58,03	0 6,7	5 20 51,9
Nov. 21	2	Em.	2 15 16	7 35 49,93	5 20 33,93		
28	2	Em.	4 52 56	10 13 17,72	5 20 21,72		
Dec. 3	1	Em.	5 2 44	10 23 31,81	5 20 47,81	+0 23,8	5 21 11,6
1811.							
Jan. 4	1	Em.	1 41 2	7 1 30,77	5 20 28,77	+1 0,3	5 21 28,8
11	1	Em.	3 36 33	8 57 7,86	5 20 34,86	+1 0,3	5 21 35,2
24	2	Em.	1 52 15	7 12 43,12	5 20 28,12	+1 6,0	5 21 34,1
31	2	Em.	4 29 15	9 49 19,04	5 20 4,04	+1 6,0	5 21 10
Feb. 25	2	Em.	1 37 40	6 57 57,54	5 20 17,54	+1 6,0	5 21 23,5
March 29	2	Em.	1 20 10	6 39 58,61	5 19 48,61	+1 6,0	5 20 54,6
Oct. 12	1	Im.	9 37 4	14 58 22,99	5 21 18,99	+0 2,4	5 21 21,4
Nov. 11	2	Im.	9 44 25	15 6 54,36	5 22 29,36	—0 56,1	5 21 33,3
	1	Im.	11 39 6	17 0 15,28	5 21 9,28	+0 5,6	5 21 14,9
Dec. 6	1	Im.	6 16 55	11 37 43,76	5 20 48,76		5 20 54,3
	2	Im.	6 52 49	12 14 23,57	5 21 34,57	+0 5,6	5 20 54,6
31	2	Em.	6 46 28	12 7 17,03	5 20 49,03		
1812.							
Jan. 14	1	Em.	6 56 38	12 17 6,94	5 20 28,94	+0 19,0	5 20 47,9
21	1	Em.	8 51 23	14 11 57,31	5 20 34,31	+0 24,0	5 20 54,3
Feb. 1	2	Em.	6 35 36	11 55 30,48	5 19 54,48		
6	1	Em.	7 9 59	12 30 33,79	5 20 34,79	+ 32,4	5 21 7,2
22	1	Em.	5 29 11	10 49 44,04	5 20 33,04		5 21 5,4
26	2	Em.	3 47 9	9 7 9,23	5 20 0,23		
March 2	1	Em.	1 53 17	7 13 47,50	5 20 30,50	+ 16,8	5 20 47,3
16	1	Em.	5 44 6	11 4 46,49	5 20 40,49		5 20 57,3
25	1	Em.	2 8 26	7 29 28,86	5 21 2,86		5 21 19,7
Oct. 7	1	Im.	10 57 17	16 19 9,65	5 21 52,65		
30	1	Im.	11 6 0	16 27 5,50	5 21 5,50		
Nov. 4	2	Im.	8 42 5	14 3 59,53	5 21 54,53	—4 6,8	5 21 7,7
11	2	Im.	11 16 57	16 38 46,31	5 21 49,31		5 21 2,5
Dec. 15	1	Im.	11 22 43	16 43 50,24	5 21 7,24		5 21 6,1
17	1	Im.	5 51 3	11 12 9,32	5 21 6,32	— 1,1	5 21 5,2
31	1	Im.	9 38 4	14 59 12,46	5 21 8,46		5 21 7,4

Date.	Satellites.	Immersions or Emersions.	Mean Time by the Nautical Almanac.	Mean Time observed at Madras.	Longitude of Madras by Tables.	Difference of Tables.	Longitude of Madras.
1813.			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
Jan. 2	1	Im.	4 6 30	9 27 13,81	5 20 43,81		
7	2	Im.	8 2 42	13 23 46,62	5 21 4,62		
	1	Im.	11 31 42	16 52 36,75	5 20 54,75		
25	2	Em.	5 27 49	10 48 42,74	5 20 53,74		
Feb. 1	2	Em.	8 4 53	13 25 26,74	5 20 33,74		
10	1	Em.	4 47 55	10 8 56,35	5 21 1,35	+ 8,5	5 21 9, 8
19	2	Em.	2 38 41	7 59 6,12	5 20 25, 1	+ 11,9	5 20 37, 0
26	1	Em.	3 5 9	8 26 26,55	5 21 7, 5		5 21 16, 0
March 5	1	Em.	4 59 53	10 20 37,67	5 20 44, 6	+ 8,5	5 20 53, 1
12	1	Em.	6 54 34	12 15 21,18	5 20 47, 2		5 20 55, 7
21	1	Em.	3 17 58	8 39 15,40	5 21 17, 4	+ 8,5	5 21 25,9
23	2	Em.	2 26 47	7 47 29,87	5 20 42,87	+ 11,9	5 20 54,8
30	2	Em.	5 4 11	10 25 32,22	5 21 21, 2	+ 11,9	5 21 33, 1
April 24	2	Im.	2 14 45	7 35 41,48	5 25 56, 4	+ 11,9	5 21 8, 3
29	1	Em.	1 50 9	7 11 1,16	5 20 52, 1	+ 8,5	5 21 0, 6
May 22	1	Em.	2 4 2	7 25 22,66	5 21 20, 6	+ 8,5	5 21 29, 1
26	2	Em.	1 59 3	7 19 57,82	5 20 52, 8	+ 11,9	5 21 4, 7
Oct. 26	1	Im.	12 14 6	7 35 28,47	5 21 22, 4	— 20,9	5 21 1, 5
Dec. 20	1	Im.	8 53 23	14 14 56,75	5 21 33, 7	— 22,2	5 21 11, 5
1814.							
Jan. 1	2	Im.	6 46 56	12 8 56,42	5 22 0, 4	— 50,8	5 21 0, 6
19	1	Im.	10 55 16	16 16 32,23	5 21 16, 2	— 15,6	5 21 0, 6
28	1	Im.	7 17 12	12 38 36,01	5 21 24, 0		5 21 8, 4
Feb. 2	2	Im.	6 23 34	11 45 37,82	5 22 3, 8	— 59,5	5 21 4, 3
4	1	Im.	9 10 49	14 31 59,37	5 21 10, 3		5 21 22, 2
6	1	Im.	3 39 12	9 0 10,68	5 20 58, 6	+ 12,0	5 21 10, 6
13	1	Em.	5 32 56	10 54 6,43	5 20 10, 4		5 21 22, 4
March 10	1	Em.	2 25 54	7 46 46,83	5 20 52, 8		5 21 4, 8
17	1	Em.	4 19 58	9 41 10,77	5 21 12, 7	+ 12,0	5 21 24, 7
24	1	Em.	6 14 9	11 35 39,80	5 21 30, 8	+ 12,0	5 21 42, 8
31	2	Em.	6 0 27	11 21 8,95	5 20 41, 9		
April 2	1	Em.	2 37 1	7 58 20,97	5 21 19,97	— 0,6	5 21 18, 4
9	1	Em.	4 31 24	9 53 0,91	5 21 36, 9		5 21 36, 3
May 2	1	Em.	4 43 36	10 4 57,03	5 21 21, 0	— 0,6	5 21 20, 4
	2	Em.	4 48 33	11 10 6,13	5 21 33, 0	. .	5 21 32, 5
June 10	2	Em.	3 14 12	8 35 10,97	5 20 59, 0	— 0,6	5 20 58, 4
Nov. 7	1	Im.	11 29 44	6 50 48,62	5 21 4, 6		
1815.							
Jan. 31	1	Im.	10 11 51	15 33 4,95	5 21 13, 9	— 17,3	5 20 56, 6
Feb. 7	1	Im.	12 5 20	17 26 34,74	5 21 14, 7		5 20 57, 4
9	1	Im.	6 33 46	11 54 59,62	5 21 13, 6		5 20 56, 3
10	2	Im.	10 15 53	15 37 59,42	5 22 6, 4	—1 9,7	5 20 56, 7
15	1	Im.	4 49 19	10 10 28,43	5 21 9, 4	— 17,3	5 20 52, 1
28	2	Im.	4 40 42	10 2 46,48	5 22 4, 4	1 9,7	5 20 54, 7
March 4	1	Im.	6 43 2	12 4 12,23	5 21 10, 2	17,3	5 20 52, 9
14	2	Im.	9 49 39	15 11 41,74	5 22 2, 7	1 9,7	5 20 53, 0
Apri 3	1	Em.	10 57 53	16 19 38,57	5 21 45, 5	27,6	5 21 17, 9
12	1	Em.	7 20 28	12 42 1,54	5 21 35, 5	27,6	5 21 7, 9
April 14	1	Em.	1 49 0	7 10 18,98	5 21 19, 0	27,6	5 20 51, 4
19	2	Em.	1 18 54	6 40 37,22	5 21 43, 2	1 3,0	5 20 40, 2
28	1	Em.	5 37 27	10 58 56,64	5 21 29, 6	27,6	5 21 2, 0
May 5	1	Em.	7 31 48	12 53 26,69	5 21 38, 6		5 21 11,11

The eclipses from 1805 to 1811, were observed during my absence in England; Captain WARREN, of His Majesty's 33d regiment, acting for me.

By correspondent Eclipses of the Satellites of Jupiter at Madras and Greenwich, from 1803 *to* 1815.

Date.	Satellites.	Immersions & Emersions.	Mean Time at Madras.	Mean Time at Greenwich.	Longitude.
1810.			h. m. s.	h. m. s.	
Aug. 22	1	Im.	16 44 2,80	11 22 47,5	5 21 15, 3
1811.					
Nov. 20	1	Im.	13 22 0,47	8 0 57,3	5 21 3,17
1812.					
Nov. 22	1	Im.	16 35 23,14	11 14 3,5	5 21 19,64
1813.					
March 12	1	Em.	12 15 21,18	6 54 12,3	5 21 8,88
1814.					
Feb. 4	1	Im.	14 31 59,37	9 10 53,4	5 21 5,97
April 7	2	Em.	13 58 34,63	8 37 6,8	5 21 27,83
1815.					
Feb. 7	1	Im.	17 26 34,74	12 5 37,3	5 20 57,44
March 14	2	Im.	15 11 41,74	9 50 48,7	5 20 53,04
April 3	1	Em.	16 19 38,57	10 58 3,2	5 21 35,37

RESULTS

By the First Satellite.

Immersions.			Emersions.		
h.	m.	s.	h.	m.	s.
5	21	4,4	5	20	46, 3
	21	6,7		20	40, 6
	21	16,9		20	25, 0
	20	45,2		21	24, 7
	20	51,9		21	24, 2
	21	21,4		21	4, 3
	21	14,9		20	48, 4
	20	54,3		21	4, 1
	21	0,1		20	58, 1
	21	5,2		20	48, 8
	21	7,4		20	48, 4
	21	1,5		20	51, 7
	21	11,5		20	55, 8
	21	0,6		21	2, 4
	21	8,4		21	3, 3
	21	22,3		21	5, 5
	21	10,6		21	7, 5
	21	22,4		21	12, 8
	20	56,6		20	59, 9
	20	57,4		21	11, 8
	20	56,3		21	15, 2
	21	52,1		21	11, 6
	20	52,9		21	28, 2
				21	35, 2
Mean	5	21 3,50		20	47, 9
				20	54, 3
				21	7, 2
				21	5, 4
				20	47, 3
				20	57, 3
				21	19, 7
				21	9, 8
				21	16, 0
				21	53, 1
				20	55, 7
				20	25, 9
				20	0, 6
				20	29, 1
				21	4, 8
				21	24, 7
				21	18, 4
				21	36, 3
				21	20, 4
				20	58, 4
				20	17, 9
				21	7, 9
				20	51, 4
				20	2, 0
				21	11, 0
			5	21	6,86
			5	21	3,50
			Mean	5	21 5,18

By the Second Satellite.

Immersions.			Emersions.		
h.	m.	s.	h.	m.	s.
5	20	59, 4	5	21	20, 5
	20	34, 4		21	33, 4
	21	33, 3		21	3, 2
	21	7, 7		21	8, 3
	21	2, 5		24	26, 4
	21	0, 6		21	9, 3
	21	4, 3		21	5, 5
	20	56, 7		21	3, 7
	20	54, 7		21	34, 1
	20	53, 0		21	10, 0
				21	23, 5
5	21	0 67		20	54, 6
				20	54, 6
				20	37, 0
				20	54, 8
				21	33, 1
				21	8, 3
				21	4, 7
				21	32, 5
				20	40, 2
			5	21	9,88
			5	21	0,67
			Mean	5	21 5, 3

RESULTS

By the correspondent Eclipses from 1803 *to* 1816.

h.	m.	s.
5	21	15,30
	21	3,17
	21	19,64
	21	8,88
	21	5,97
	21	27,83
	20	57,44
	20	53,04
	21	35,37

Correspondent Eclipses from 1787 *to* 1816.

Immersions.				Emersions.			
h.	m.	s.		h.	m.	s.	
5	20	22, 8		5	21	25, 8	
	21	5, 5			21	30, 3	
	20	40, 8			21	43, 5	
	20	35, 3			21	5, 3	
	21	5, 3			21	5, 8	
	21	0, 0			21	44, 3	
	20	55, 7			21	27, 5	
	21	13,			21	5, 7	
	20	52,			21	34, 5	
	21	15, 3			22	2, 4	
	21	3,17			21	17, 7	
	21	19,64			21	19, 4	
	21	5,97			21	8,88	
	20	57,44			21	27,83	
	20	53,04			21	35,37	
5	20	57,66	Mean.	5	21	26,28	Mean.
	21	26,28	Emersions.				
	21	11,97	Mean.				

Longitude of the Madras Observatory from the whole of the foregoing Observations. By correcting the Tables:

First and Second Satellites.

	h.	m.	s.	h.	m.	s.
Eclipses reduced to Madras,	5	21	6,85			
Observed at Madras, but not with the same telescope,	5	21	8,42	Mean 5	21	7,77
Observed at Madras to 1802, with the same description of telescope,				5	21	7,72
From 1802 to 1815, with telescopes of the same powers,				5	21	5,24
Mean by correcting the tables,				5	21	6,78
By correspondent observations at Greenwich, from 1787 to 1816,				5	21	11,97
Mean, or longitude of the Observatory,				5	21	9, 4
East of Greenwich				80	17	21

Fort St. George Church-steeple is 2′ 21″ east of the Observatory; the longitude of the Steeple is therefore 80° 19′ 42″ east.

The longitude of the lunar observations before alluded to, about 800 in number, taken between the years 1787 and 1792* at different parts of Madras and at Coringa, and reduced to the Observatory, is 80° 20′ 16″,5 east; and therefore, according to the eclipses, 2′ 55″,5 too great.

* By the late Honourable W. Petrie, Esq. the late Mr. Topping, and myself: the Coringa observations by Mr. Topping.

I shall now proceed to give some information respecting the latitude of the Observatory. The height of the pale at Madras being only 18 degrees, the method by circumpolar stars cannot be used; and the latitude has been found by meridianal observations of the sun and stars north and south of the zenith, taken with the sextant, a circular instrument of 18 inches diameter by TROUGHTON, and the zenith sector used in Colonel LAMBTON's Survey.

The results were as follow:

Latitude of the Madras Observatory by observations of stars with the sextant,	-	-	° ′ ″ 13 4 8,606
With the circular instrument, stars north and south of the zenith,	-	13° 4′ 11″,894	
Second set,	-	- - 13 4 6 ,770	13 4 9.332
Stars near the zenith,	-	- -	13 4 7.917
Correspondent observations of the sun at Greenwich,	-	- - - -	13 4 11,163
Observations of the sun,	-	- -	13 4 5.363
Mean by the circular instrument and sextant,	-	- - - -	13 4 8,476
By observations with the zenith sector, stars north and south of the zenith,	-	- - 13 4 11 , 95	
Sun,	-	- - - 4 5 , 15	13 4 8, 55
Mean latitude by Mr. GOLDINGHAM's observations,	-	- - }	- - 13 4 8,513

During my absence in England, I find the zenith sector was again brought to the Observatory, there being some doubt, as it would seem, of the correctness of the foregoing

conclusion.* The following are the results of the observations, which are very numerous.

By Captain WARREN'S observations with the zenith sector.

Latitude, stars north and south of the zenith,

Table II. of the Records, -	13° 4′ 15″,074		
Table III. - - -	4 13 ,717	° ′ ″	
Mean by stars - - -		13 4 14,395	
by the sun - - - -		4 5,483	
Mean latitude by Captain WARREN'S Observations - - -		13 4 9,989	
Mean latitude by both; Mr. GOLDINGHAM with the circular instrument and sextant - - -		13 4 8,479	
Zenith sector, Stars - - -		11,950	
Sun - - -		5,150	
Captain WARREN'S, zenith sector,			
Stars - - - - - -		14,395	
Sun - - - - -		5,483	
Mean latitude† -		13 4 9,1N.	

It would therefore appear that very little (if any) additional light had been thrown upon the subject by the latter obser-

* Owing, probably, to the difference between the latitude by the sun, and that by the stars; a difference, however, much greater in the latter observations than in the others; and, as I have observed, not easily accounted for. I hope, however, that the Observatory will be furnished with a large circle, which, besides enabling us to obtain other valuable information, may lead to a discovery of the cause of this difference.

† This result is found from about 700 observations.

vations; those formerly taken,* differing from the mean of the whole little more than half a second. The mean of the whole, 13° 4′ 9″,1,† may therefore be considered, for the present at least, the latitude of the Observatory.

It will be observed, that the meridianal observations of the sun give a different result from those of the stars. In mine, this difference is about 4 seconds less than by the stars; in the second series, the difference is nearly 9 seconds also less than by the stars; a difference not easily accounted for. It is curious however to remark, that the correspondent meridianal zenith distances of the sun at Greenwich, give a result *greater* than the mean latitude by the sun 5″,85; and the same elements are used in both cases, with the exception of declination.

Of the Longitude‡ of Calcutta.

By a series of correspondent eclipses of the satellites of Jupiter, taken in Fort William, by the late Lieutenant-Colonel COLEBROOKE, of the Bengal Establishment; the telescope at the Observatory, and that used at Calcutta, being in all

* The observations being so numerous, in the conclusions now drawn from mine, the method of selection used with the eclipses has been adopted, viz. by taking a mean of the whole, and then rejecting those results which differ more than the power of the instrument would seem to warrant: this, with the sextant, I have considered 10″, with the circular instrument 8″, and with the zenith sector 4″. The latitude formerly deduced stood thus: mean of the observations with the zenith sector 13° 4′ 8″,55; with the circular instrument 13° 4′ 8″,40; with the circular instrument and sextant 13° 4′ 8″,5; mean 13° 4′ 8″,48 or not one-tenth of a second different from the result now deduced by these observations.

† Fort St. George Church Steeple is 36″ N. of the Observatory. Latitude of the Steeple is therefore 13° 4′ 45″ N.

‡ According to RENNELL's Memoir, the Longitude by the Honourable THOMAS HOWE, was 88° 33′; by mean of four observers, 88° 27′ 45″.

respects alike, the difference of longitude was found to be 8° 6′ 18″.

Longitude of the Madras Observatory	80°	17′	21″
Calcutta (Fort William) E. of the Observatory	8	6	18
Longitude of Fort William*	88	23	39 E.

Of the Longitude of Bombay.

In the year 1791, being at Bombay, on my way from England to Madras, and aware that great doubts existed as to the longitude of that important commercial station,† I proposed taking some observations, while detained there for a passage to this coast, with the view of assisting in the determination of the question; and accordingly commenced observing the eclipses of the satellites of Jupiter, and a series of lunar observations; taking also at the same time a sufficient number of meridianal observations for determining the latitude. The results were as follow:

By the mean of about 160 lunar observations with a sextant by Troughton, the longitude of the place of observation at Bombay was 72° 57′ 39″ E.; and by the mean of 180 lunar observations with a sextant, having Ramsden's name on it, the longitude of the place was 72° 57′ 55″. The mean of both was 72° 57′ 47″.

But it would appear from the lunar observations taken at

* The latitude of Calcutta is considered 22° 33′ N.

† Mr. Howe's longitude, 72° 38′, appearing at the time to be considered the most correct; but Captain Huddart had placed it more than a quarter of a degree farther to the eastward.

Madras, that the tables about that period gave 2′ 55″,5 too much. If, therefore, this quantity be subtracted from the result, the longitude will be only 72° 54′ 5″ E.

More than 30 eclipses of the satellites of Jupiter, immersions as well as emersions, were also observed; and by allowing for the difference of the tables at that period by comparisons with Greenwich observations, the longitude of the place of observations, by the mean of the first and second satellites, was 72° 53′ 26″.

Also, by a very good chronometer, the rate of which I found before leaving Bombay, and after my arrival at Madras* (having been about 16 days on the passage), the difference of the longitude between the place of observation at Bombay and the Madras Observatory is . 7° 24′ 12″

This gives for the longitude of the former 7° 53′ 9″

The longitude of Bombay (place of observation) by the lunar observations corrected . .	72 54 52
By eclipses of the satellites of Jupiter, Tables corrected	72 53 26
By the difference of longitude by the chronometer	72 53 9
Mean	72 53 49

But the place of observation was 54 seconds of a degree west of Bombay Church, and 13 east of the Light House; therefore the longitude of the Church, by these observations, is 72° 54′ 48″, and of the Light House, 72° 53′ 36″ east of Greenwich.†

* Rate on quitting Bombay + 40″,43
On my arrival at Madras + 40, 34.

† The difference of meridians between Madras and Bombay Churches, according to these deductions, is 7° 24′ 59″.

The latitude was found by 32 meridianal observations of the sun and stars, north and south of the zenith, taken with the two sextants, and an artificial horizon. The height of the thermometer and that of the barometer was noted at the time of each observation, and the correction on this account was applied to the refraction. The declinations were also corrected for aberration, &c. and the results were:

By 16 observations with TROUGHTON's instrument	18° 57′ 43″,5
By 16 observations with RAMSDEN's	18 57 43 ,8
Mean latitude	18 57 44 N.

The place of observation was 1′ 37″ north of the Church, and 3′ 19″* north of the Light House. The latitude of the Church is therefore 18° 56′ 7″, and of the Light House 18° 54′ 25″ N.

It may be useful to remark upon a difference with RAMSDEN's sextant, in the results by the objects north, and by those south of the zenith in observing for the latitude; and also the difference in the results by the lunar observations east and west of the moon. The instrument was most carefully examined, and the error regularly found every day by measuring the sun's diameter; yet, notwithstanding, the following differences were in the results:

In the observations for the latitude, the object being North of the zenith	18° 58′ 11″
South of the zenith	18 57 16,6
Difference †	0 55

* A survey was made for ascertaining this, as well as for the difference of longitude.

† From this difference in the results for the latitude, a difference of more than half a degree might be looked for in those of the longitude, as we find to be the case.

In the observations for the longitude, the object		
being East of the ☽ . . .	78° 15′ 4″,4	
West	72 40 46 ,2	
Difference	34 18 ,2	

The sextant by TROUGHTON, in the observations for the latitude, gave only a difference of four seconds between the results by the objects north, and those south of the zenith; and about three minutes in the observations for the longitude. Whether the differences in the results by the other sextant arose from an error in the total, or from what other cause, is not easy to determine. The instrument appeared perfect; but these results, however, show the necessity of observing objects both north and south of the zenith for the latitude; and also objects east and west of the moon for the longitude. The mean of the results thus obtained will be correct; the objects on one side, giving a longitude as much greater, as those on the other side give one as much less, than the truth. In consequence, we find that the mean latitude by RAMSDEN's instrument is only a few tenths of a second different from that by TROUGHTON's; while the mean longitude is only about sixteen seconds of a degree different.

J. GOLDINGHAM.

Madras,
6th December, 1819.

XXXI. *Of the difference of longitudes found by chronometer, and by correspondent eclipses of the satellites of Jupiter; with some supplementary information relative to Madras, Bombay, and Canton; as also the latitude and longitude of Point de Galle and the Friar's Hood.* By J. GOLDINGHAM, *Esq. F. R. S.*

Read June 27, 1822.

Of the difference of longitude found by chronometer, and by correspondent eclipses of the satellites of Jupiter.

IN my former Paper, I mentioned these methods of finding the longitude, after the distance of one point from a first and distant meridian had been correctly established, and at the same time carried both into practice; the former, in deducing the longitude of Bombay, and the latter, in the operation for the longitude of Calcutta; and I bring forward the following observations by these methods, to show what may be effected in practice by either, where the instruments are good, and the necessary precautions have been taken. The observations at Masulipatam were taken by the late Mr. TOPPING, those at the Observatory, by myself. The telescopes at both places were the same in construction and magnifying power.*

* Masulipatam is not two days sail from Madras in the S. W. monsoon.

Madras Observatory and Masulipatam Flag Staff.

By correspondent eclipses of the satellites of Jupiter.

	Satellites.	Em. or Im.	D. Long to Masulipatam Flag Staff.
1794	1	E	0° 52′ 6″
	1	E	0 48 22,5
1795	2	I	1 0 40,7
	1	I	1 8 0,7
	1	I	0 50 5,2
	2	I	0 46 53,2
	1	I	0 50 45,7
	1	E	0 57 23,2
	2	E	0 59 38,2
	1	E	0
	1	E	0 57 17,2
	1	E	0 54 41,2
	2	E	1 1 33,7
Mean . .			0 55 12
Observatory .			80 17 21
Masulipatam Flag Staff			81 12 33

By Chronometer.

	Difference of Meridians.
By the chronometers of ARNOLD's, in the year 1793	0° 55′ 48″
In the year 1794, by 2 chronometers of ARNOLD's	0 55 60
In the year 1795, by 1 chronometer of ARNOLD's	0 53 54
Mean .	0 54 54
Observatory	80 17 21

Longitude of Masulipatam Flag Staff by chronometer	81° 12′ 15″
Longitude of Masulipatam Flag Staff by the eclipses	81 12 33
By the chronometer	81 12 15
Mean	81 12 24 E

Here we find a wonderful agreement by the two methods; and the longitude of *Masulipatam*, a point of importance on the coast, may be considered correctly determined.

The following information may be deemed supplementary to that given in my former paper on the Geographical situation of the three Presidencies, and it is hoped will be found useful to navigators.

At Madras, the Fort Flag Staff is about 1″,6 north, and 2 east of the Church Steeple. This gives for the latitude of the Flag Staff 13° 4′ 47″ north, and longitude 80° 19′ 44″ E. As navigators generally take their departure from the Flag Staff, I have deemed it proper to give its situation, as well as that of the Steeple, given in the former paper.

The tide on the coast about Madras seldom rises more than 3 feet; and it is high water on the Syzigies, by my observations, at $9^h\,25^m$. The variation of the compass towards the end of 1792, on the coast, about a degree to the northward of Madras, was 1° 3′ east, by numerous observations.

At Bombay, the time of high water on the Syzigies, at the Dock Head, from several observations which I made, was $11^h\,32^m$. The pilots generally allow $11^h\,\frac{1}{2}$ as the time. The greatest rise of the tide at the Dock Head was 18 feet. This

happened at the springs near the vernal equinox of 1791; and except from particular combinations, it is never known to rise higher: as I was informed, the medium rise of the springs is about 15½ feet. The variation of the compass in the beginning of the year 1791, by the mean of many observations, I found to be 4ª′ 59″, or 4ª′ west.

During the passage from Bombay to Madras, I had an opportunity of ascertaining the latitude and longitude of Point de Galle, and of the Friar's Hood; and as the chronometer performed so well, it is to be regretted we did not see other places on the way. Point de Galle is, however, an important position to have accurately determined, and in the year 1791, the longitude deduced from different observations varied from 80° 1′ 30″ to 80° 2ª′.

On the 8th of September we saw Point de Galle Flag Staff. Three bearings of it were taken with the azimuth compass; one when it was E. 8° 24′ N.; a second when it was N. 28° 38′ E.; and the third when it was due north. The time when each bearing was taken was correctly noted; and a base, measured by the run of the ship, corrected for current, was used for finding the distances, which, at the time the first bearing was taken, was 5,5 miles, and at the time the second was taken 3,7. The ship being in the longitude of Point de Galle at the time the third bearing was taken, no distance was necessary. Altitudes for finding the longitude by the chronometer were observed about half an hour before the first bearing was taken, which was at 5ʰ 45′ p. m. apparent time. These altitudes give for the longitude of the ship at 5ʰ 10′ p. m., (by mean of two days observations after my arrival at Madras, and allowing 80° 19′ 4ª″ as the longitude of the

Church) 80° 9′ 6″, the latitude of the ship being at that time 6° 2′ 4″ N.; the difference of latitude and difference of longitude made by the ship to the times the bearings were taken, being of latitude 2′ 4″ S. and 4′ 25″ S.; of longitude 2′ 27″ E. 6′ 6″ E. and 8′ 3″ E.

The latitude and longitude of Point de Galle Flag Staff from these observations were

	Latitude.	Longitude.
By the first bearing	6° 0′ 47″ N.	80° 16′ 59″
Second .	6 0 58	80 16 57
Third . . .		80 17 9*
	6 0 50	80 17 2 E.

Three bearings of the Friar's Hood were also taken on the 10th of September with an azimuth compass: one when we were nearly in the latitude, and another when in the longitude of the Hood. The same care was taken in finding the distances, &c.; and the latitude of the Hood was found to be 7° 29′ 35″ N.; longitude 81° 36′ 3″ ¼ E.

It will no doubt be concluded, that where so much attention was paid to have the correct bearings and distances, that the meridianal observation for the latitude, and the altitudes for the longitude, were taken with a sextant:—this was the case; and two sets of the latter were always observed.

About the time the earliest of the observations mentioned in my former paper were taken, three correspondent eclipses of the satellites of Jupiter had been observed at Canton, by

* By the late authorities, Point de Galle is placed 7° 22′ 30″ E. of Bombay. Now, allowing Bombay to be in 72° 54′ 43″, as deduced in my former paper, the longitude of Point de Galle will be 80° 17′ 13″ east.

Captain HUDDART, and at Calcutta,* which gave the difference of longitude of those places 24° 54′.

Longitude of Calcutta, by the deduction in my former Paper	88° 23′ 39″
D. longitude to Canton . . .	24 54 0
Longitude of Canton	113 17 39

Captain HUDDART, by seven eclipses of the satellites, the difference in the Tables being allowed for, made the longitude of Canton 113° 19′ 7″; the mean of both is 113° 18′ 23″ east of Greenwich.

J. GOLDINGHAM.

* This information was given me at the time by Mr. J. LINDLEY, formerly Assistant at the Royal Observatory.

XXXII. *Observations on the genus Planaria.* By J. R. Johnson, *M. D. F. R. S.*

Read June 27, 1822.

I beg leave to lay before the Royal Society a few observations on the genus Planaria, to which I have been more immediately led, from two or three species having been already described in the Linnæan Transactions under the genus Hirudo.

I shall confine my remarks in the present paper to the following species: *P. cornuta, P. torva, P. brunnea, and P. lactea*, intending to give, at some future period, the farther history of this singular and extensive genus.

P. cornuta.

Planaria depressa, oblonga, nigro-cinerea, antice tentaculis instructa.

Long. 4 *lin. Lat.* 2 *lin.*

Body, blackish brown, convex above, flat beneath; fore part exsertile and retractile, with a tentaculum or feeler projecting on each side of the head; tail pointed; two ventral foramina; numerous eyes.

P. torva.

Planaria depressa, oblonga, cinerea, subtus albida, iride alba.

Long. 6 *lin. Lat.* 2 *lin.*

Body, brown on the back; grey or whitish on the belly; slightly convex above, plain beneath; truncate before, pointed behind; iris, white; two ventral foramina; eyes, two, black, in a white areola.

P. brunnea.

Planaria oblonga brunnea, linea longitudinali nigra.

Long. 5 *lin. Lat.* 2½ *lin.*

Body, dark brown, with a black dorsal longitudinal line; convex above, plain beneath; obtuse before, slightly pointed behind; two ventral foramina; numerous eyes.

P. lactea.

Planaria depressa, oblonga, alba, antice truncata.

Long. 6 *lin. Lat.* 2½ *lin.*

Body, milky-white, or cream-colour; slightly convex above, plain beneath; truncate before, sharply pointed behind; lateral margin acute, pellucid; two ventral foramina; eyes, two, deep black.

These little animals, which are delineated of their natural size, in Fig. 1, 2, 3, 4, are generally found in slow streams, assembled together in clusters, attached to the roots, or under surfaces of the leaves of aquatic plants, pieces of wood, stone, &c. In a state of rest, they are of a circular or spherical shape; when in motion, linear. They are remarkably quick and rapid in their movements, and contract and lengthen themselves with the greatest facility. From their great con-

tractility, the body consisting of a series of annular muscles, they are enabled to assume almost every variety of figure. In traversing a smooth surface, they have usually a gliding or snail-like motion, leaving a viscous tract behind; but when the surface is rugged, they quit this gliding motion, and move forward like the leech, alternately attaching the head and tail: they do not, however, as far as I can perceive, fix the tail; they simply draw it up, when the body is firmly retained by the strong glutinous substance with which it is invested.

The *Planariæ* are often seen traversing the surface of the water in an inverted position, and with the same facility as the *Glossoporæ*, described in a former paper. When gliding along, they move the head from side to side with rapidity, but on coming suddenly into contact with any hard substance, they immediately retract, or draw in the fore-part of the body, somewhat like the *Limax*.

On visiting the rivulet, from which I was in the habit of taking these animals for the experiments I am about to relate, I was surprised to find a large body of them *(P. torvæ)* proceeding against the current, gliding over its sandy bottom, keeping the same order as ants when passing from one of their establishments into another, and occupying a space of about twelve inches in length by two in breadth. This regular movement I observed two or three days in succession. The weather being at this time extremely temperate, had, doubtless, induced them to quit their several hiding places; but I could not discover the purport of this proceeding.

The *Planariæ* are so gelatinous and pulpy in their texture, as scarcely to allow of any very accurate dissection. I may

however remark, that the body consists of one common cavity, with diverging lateral cells or branches, destined to contain the nutriment; being in this arrangement very analogous to the stomach of the medicinal leech.

In the absence of food, the *interanea* of these animals cannot be distinctly seen. In the *P. lactea*, they present a beautifully arborescent appearance, as delineated, highly magnified, in fig. 17. Sometimes they are of a deep purple colour, at other times of a brilliant red or dark brown, depending upon the nature of the food taken.

The ventral apertures, which are two in number, and circular, are very evident in each of the Planariæ above described, but more particularly so in the *P. torva* (fig. 6); the upper orifice gives passage to a long flexible tube, the lower one conducts to the ovarium. Of the use of this tube, which the *Planariæ* frequently project, and which nearly equals the body in length, I remained for some time ignorant, when it was discovered to me by mere accident.

Being desirous of ascertaining the proper food of these animals, as they were languishing in confinement, I threw in among them a variety of aquatic insects, worms, &c. One of them (*P. cornuta*), after the lapse of a few minutes fastened upon a worm, immediately projecting and affixing this tube: the worm being in this way closely retained, other *Planariæ* came forward to its assistance, and thus completely overpowered it. It is astonishing with what obstinacy they maintained their hold, notwithstanding the writhings and contortions of the worm to effect its escape.

The *Planariæ* seldom attack the worm openly, seemingly aware of the difficulty of thus overcoming it, but seize upon

it, as it were, by stealth, gliding gently underneath it, and then projecting and affixing this organ, keeping a firm hold until they have concluded their repast. I have seen, at times, nearly a dozen preying upon a solitary worm. *Fig.* 16 represents the *P. cornutæ* thus engaged; at *a* and *b* this tube is seen projected. What however surprised me, was, to observe these animals, for a considerable time after, moving the head freely from side to side, still keeping the tube firmly affixed. As the worm was yet living, I imagined the sole use of this organ to be, that of effectually securing their prey. To be convinced of this, I gave them a dead worm, which they fastened upon in like manner: the head or mouth of these animals remaining, as in the former instances, unattached. This singularity could not fail of interesting me, since it appeared they received their food by this organ, and not by the mouth.

To remove any doubt which might still exist, I cut off the head of one of the *P. lacteæ*, and to the headless body presented, a day or two after, an earth-worm, upon which it soon affixed, and then inserted this tube; procuring in this way an abundant supply, sufficiently evidenced by the *interanea*, which were not before visible to the naked eye, becoming immediately conspicuous. Another *P. lactea* attached itself also to a portion of a large earth-worm, keeping its head the whole time detached, as delineated in *Fig.* 15. Here, also, the *interanea* were filled, affording an indisputable proof of nourishment having been taken in by this organ alone.

From a number of other experiments I found, that when the *Planariæ* were perfect animals, this organ was, while taking their food, in constant use; but in the event of their

being naturally or artificially divided, to which we shall soon advert, or their losing this tube, which was not unfrequently the case, they took their usual sustenance by the mouth.

I should not have entered into this detail, were I not apprehensive that I should be considered as labouring under some mistake, since it seems so contrary to the generally established law of nature, that an animal, furnished with a proper mouth, should receive its food by another organ, and that organ placed as near to the tail as to the head. Such, however, is the case; and the proofs advanced, will, I trust, be sufficiently convincing. In addition, I have to observe, that MÜLLER, after alluding to this organ, in his description of the *P. lactea*, says, "*Organum hoc nutrimento capiendo inservire vix dubitari potest.*"

Although I have repeatedly seen the young of the *P. torva* and *P. cornuta*, I have not yet been able to determine whether they are oviparous or viviparous, which I conceive to have arisen from having begun my enquiries late in the season. With respect to the *P. lactea* and *P. brunnea*, they are oviparous, producing eggs, within a membranous capsule, each capsule containing (at least those of the *P. lactea*), from 3 to 8 young.

One of the *P. lactea* deposited a capsule, July 8, which, on the 10th of August (33 days), produced 8 young.

Considering the size of the *P. lactea*, the capsule is of great magnitude, occupying nearly one-fourth of the body, forming a remarkable protuberance on the back. (fig. 5).

Six capsules were deposited by the *P. brunnea*, August 12, but of a size much less than those of the *P. lactea*; one only was productive (September 5th); it contained 2 young.

From a neighbouring pond I collected several capsules of this species, which I found attached to the under surface of the leaves of the *ranunculus aquatilis*, and found them to produce as follows:

July 26, 16 young from 5 capsules.
August 8, 25 ——— 7 ———
——— 12, 10 ——— 3 ———

making an average of three young to each capsule.

The young, on their immediate exclusion from the egg, are of different sizes, and remarkably active. I have sometimes seen them after taking food, so stretched, as nearly to double their previous size, a convincing proof of the great extent of the abdominal cavity.

A very singular part of the history of these animals, and what does not appear yet to have been noticed, is, that they have a double mode of perpetuating their species; 1st. by capsules, which, as we have seen, contain several young; 2ndly, by a *natural* division of the body into two portions, the head part reproducing a tail, and the tail a head, in about fourteen or more days, depending upon the state of the atmosphere.

Fig. 7 and 8 represent the *Planariæ* when about to divide to form two distinct animals. On the third day the separation of the head from the tail usually takes place, as at *fig.* 9. When undergoing this division, they remain for the most part stationary, keeping the head firmly affixed, twisting round the tail, from time to time, with a view of lessening its adhesion, and thus more readily effecting its disunion. Almost immediately after the head is liberated, it is seen to move with all the freedom of the unmutilated, perfect animal. The

tail generally remains attached, and only occasionally shifts its situation; but if touched, it moves with nearly the same quickness as the anterior extremity, preserving an uniform gliding motion.

This property, which is common to the several *Planariæ* enumerated in this paper, is most strongly marked in the *P. cornuta.*

The reproductive power of these animals, when divided, is alluded to by MÜLLER, SHAW, and others. Indeed it could scarcely be expected, after the public attention had been excited by the astonishing reproduction of the *Polype*, even when cut or divided into several minute pieces, that the reproduction of an animal, so open to common observation as the *Planaria*, should long have remained unnoticed.

The following experiments show to what a degree this reproductive power extends.

July 8. I divided several of the *Planariæ* into two equal parts: the head and tail portions of the *P. torva* were reproduced in 14 days; those of the *P. cornuta* in 17 days; of the *P. brunnea* in 15 days; and of the *P. lactea* in 19 days.

July 30. An equal number of the *P. cornutæ* and *P. lacteæ* were divided, transversely, a little below the eyes. The heads of the former were renewed in 11 days, of the latter in 14 days. The head portions were a long time in recovering the tail: in 16 days they were as delineated in *fig.* 10 and 11. As the *P. lactea* had recently taken food, the *interanea* are also exhibited.

August 9. The *Planariæ* were now divided into three equal parts: the middle portions were observed to reproduce heads and tails by the 25th (16 days). *Fig.* 12 represents, under

a magnified form, the *P. brunnea*, and *fig.* 13, the *P. cornuta*, with renewed heads and tails. The head portions show, very distinctly, a circular-like range of black dots close to the margin, which I conjecture to be the eyes; and the tail portions show the renewal of the abdominal tube.

July 30. The *Planariæ* with newly formed heads, mentioned in the first experiment, were again divided, and reproduced by August 25 (26 days). At the time of the former experiment these animals were strong and vigorous, which sufficiently accounts for the difference of time in the reproduction. *Fig.* 14 represents the head of one (*P. torva*) highly magnified, showing the eyes in the centre of the white areola.

I afterwards divided these animals into four, five, and even six parts; and what was extremely singular, each part seemed to possess the properties of a perfect animal, moving about in the water in the same gliding manner as before the separation. But when thus divided, they seldom retained their vitality beyond a few days, or if any portion did, perchance, recover its lost parts, it was a process extremely slow and tedious.

To render these experiments successful, it will be necessary to divide the *Planariæ* immediately on their being taken from their native haunts, for, when long confined, they lose their activity, and are ill calculated for the purpose. I should have observed, that, although the lost parts are generally reproduced in about a fortnight, yet a month or more is required before they take on the precise form and colour of the original animal.

The *Planariæ* with renewed organs were placed in a vessel

by themselves, and two or three, notwithstanding their previous artificial division, divided naturally, as at *fig.* 9.

Having frequently noticed several full-grown *Planariæ* with the head and tail of a greyish white, the rest of the body being brown or black, I should, without the knowledge of this re-productive property, have been led to consider them a distinct species.

In reference to the *P. lactea*, MÜLLER says; "*Unicum exemplar cujus cauda in duos lobos acutos fissa erat reperi, aliud absque oculis, et interaneis conspicuis, mere lacteum; cæterum idem.*" It seems he was not aware, or I think he would have noticed it, that the circumstance of these *Planariæ* being without eyes, &c., was occasioned by their having undergone a natural division, in order to their becoming distinct and separate animals.

J. R. JOHNSON, M. D. F. R. S. and L. S.

Bristol,
September 24, 1821.

EXPLANATION OF PLATE XLIX.

Fig. 1. *P. cornuta*, front view, natural size.
2. *P. torva*, Do. Do.
3. *P. brunnea*, Do. Do.
4. *P. lactea*, Do. Do.
5. Do. with its egg or capsule.
6. Back view of *P. torva*, showing the ventral foramina.
7. *P. cornuta*, about to undergo a natural division.
8. *P. torva*, Do. Do.

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

Fig. 7.

Fig. 8.

Fig. 9.

Fig. 15.

Fig. 16.

Fig. 14.

Fig. 10.

Fig. 11.

Fig. 17.

Fig. 13.

Fig. 12.

9. Head and tail of *P. cornuta*, naturally divided.
10. Head of *P. cornuta*, with tail partly renewed.
11. Head of *P. lactea*, Do. Do.
12. *P. brunnea*, with renewed head and tail.
13. *P. cornuta*, Do. Do.
14. Head of *P. torva*, highly magnified, showing the eyes in the centre of the white areola.
15. *P. lactea* attached to a portion of a large earth-worm, with the abdominal tube inserted.
16. *P. cornutæ* preying upon an aquatic worm, showing at *a* and *b* the abdominal tube.
17. *Interanea* of *P. lacteæ*, highly magnified, showing its arborescent ramifications.

XXXIII. *Some experiments and researches on the saline contents of sea-water, undertaken with a view to correct and improve its chemical analysis.* By Alexander Marcet, *M. D. F. R. S. Honorary Professor of Chemistry at Geneva.*

Read June 27, 1822.

In a paper on the temperature and saltness of various seas, which the Royal Society did me the honour to publish in their Transactions for the year 1819, I threw out a conjecture, that the sea might contain minute quantities of every substance in nature, which is soluble in water. For the ocean having communication with every part of the earth through the rivers, all of which ultimately pour their waters into it; and soluble substances, even such as are theoretically incompatible with each other, being almost in every instance capable of co-existing in solution, provided the quantities be very minute, I could see no reason why the ocean should not be a general receptacle of all bodies which can be held in solution. And although it will appear from the following account, that I have been unsuccessful in some of my attempts to prove the truth of this conjecture, it may fairly be ascribed either to a want of sufficient accuracy in our present methods of chemical analysis, or of the requisite degree of skill in the operator.

Some time after the communication to which I have just referred, an extraordinary statement was pointed out to me, upon the authority of Rouelle, a French chemist of the last century, from which it appeared that mercury was contained

in sea salt:* and I saw soon after in the '*Annales du Musée*,' Vol. VII. a paper by the celebrated chemist PROUST, who, in a great measure, confirmed that statement, by announcing that he had found traces of mercury in all the specimens of marine acid which he had examined.

Improbable as the fact appeared, I thought it worth while to repeat the experiment, and to take that opportunity of making some collateral researches upon other substances, much more likely than mercury to be discovered in sea water.

For this purpose I availed myself of the kindness of my friend Mr. JOHN BARRY,† who happened to be in the vicinity of Portsmouth, to supply me with specimens of sea-water, carefully concentrated upon the spot, in vessels of Wedgwood ware, and with scrupulous attention to cleanliness in the process. Accordingly he was so obliging, as not only to send me a quantity of brine evaporated under his own eye, in the manner just mentioned, but he also collected for me a valuable series of specimens from the salt works near Portsmouth, from all the stages of the process, so as to afford me an opportunity of investigating with accuracy, all the chemical circumstances of this interesting branch of national œconomy. Finding myself, however, much pressed by time at this late period of the session, I shall, after briefly adverting to ROUELLE's supposed discovery, confine myself in this communication to a few observations which I have made on sea-water itself; keeping out of view, for the present, the topic

* See 'Journal de Médecine,' Vol. xlviii. 1777, page 322.

† Mr. JOHN BARRY, of Plough Court, inventor of a new and valuable process for preparing extracts in vacuo, &c.

of salt-making, which, however, I intend to resume at some future period, in a more complete and satisfactory manner.

I first attempted to detect mercury in a specimen of *bay-salt*, such as is obtained in the salt-works near Portsmouth, by spontaneous evaporation. This variety of salt forms large crystals, but is always more or less contaminated by earthy matter, which gives it a dirty appearance. It has, probably, a general resemblance to the French *Sel de Gabelle*, which is more impure still, though, I believe, obtained in a similar manner.*

Eight ounces of this salt were put into a coated retort connected with a receiver, and about four ounces of nitrous acid were poured upon it. A pretty brisk action took place, which was farther increased by the application of heat; fumes of chlorine were immediately disengaged, and a reddish fluid condensed in the receiver; the heat was continued, and gradually raised in a charcoal fire till no acid or moisture any longer came over; at which time a new emission of red fumes indicated that the nitrate formed in the retort was beginning to part with its acid: minute drops of fused salt soon bedewed the upper part and neck of the retort, so as to be mistaken, at first, for a sublimate. This, however, proved to be almost solely muriate of soda; and on careful examination, it did not appear to contain the smallest atom of corrosive sublimate.

I next dissolved five or six pounds of bay-salt in water, and collected in a filter the insoluble earthy sediment, in

* The name of *bay-salt* is often applied to foreign, as well as British salt, and in general it simply denotes that the salt has been obtained by spontaneous evaporation.

which ROUELLE stated that the quick-silver was usually found. This sediment being carefully dried, and heated to redness in a coated retort, a white sublimate arose, and condensed on the neck of the retort; but this sublimate proved to be muriate of ammonia, and did not contain the smallest portion of corrosive sublimate or other mercurial salt. This sal-ammoniac, though evidently formed during the distillation from the vegetable and animal matter contained in the sediment, suggested to me the idea of looking for ammonia amongst the contents of sea-water.

I now submitted some *Sel de Gabelle*, which I had procured from Calais for the purpose, to similar experiments, and the sediment, also, was carefully examined. The result was essentially the same as with the bay-salt. After adding nitric acid to the salt, the heat was gradually pushed to redness; and when all the moisture was evaporated, a white sublimate appeared, as in the former case, which, in this instance, proved to consist almost entirely of nitrate of soda; but always without the least particle of mercurial salt, and without any muriate of ammonia.*

I therefore think myself justified in concluding that the mercury, which other chemists have detected in sea-salt or its products, must have been introduced there from some local or accidental circumstances.

In experiments upon sea-salt, or in general upon the saline contents of the sea, it is obvious that, in order to exclude sources of error, it is necessary to operate upon pure sea-

* In the former experiment the sublimate was principally muriate of soda, owing, no doubt, to the decomposition having been less complete, and the operation less gradually conducted than in the latter experiment.

water, and not upon salts obtained from it by the usual processes in the large way, these being always more or less contaminated by the clay pits in which the evaporation is carried on, by the metallic boilers, or other adventitious causes. I therefore now turned my attention to the sea-water itself, and in particular the perfectly pure and transparent specimen of concentrated brine from the Channel, which I have above mentioned. Mr. BARRY procured this water near Bembridge floating light, about two miles N. E. of the eastern extremity of the Isle of Wight, and the evaporation which it had undergone at Portsmouth, had only separated from it a quantity of calcareous matter, principally selenite.*

A few pounds of this water were evaporated nearly to dryness, at a gentle heat, so as to reduce the mother liquor to the smallest possible quantity. This liquor was suffered to drain off, and reserved for experiments, as it is in this fluid that any new ingredients are most likely to be detected.

I had suspected that some nitric salt might be found in sea-water, but in this I was disappointed. The discrimination by the shape of the crystals being in this instance scarcely practicable, the mode which I employed for detecting it, consisted in concentrating the bittern in a glass tube or retort, till it began to deposit solid matter, then adding sulphuric acid and gold-leaf, and boiling the mixture; the gold-leaf was not in

* The water, immediately on being raised from the sea, had been allowed to stand a sufficient time to deposit the earthy particles suspended in it, by which means it had become beautifully transparent. 100 pounds of the water produced only 3 grains of earthy sediment, in which I could discover nothing but carbonate of lime and oxide of iron. It is in this sediment, according to ROUELLE, that mercury is to be found. I need hardly say that I could not detect in it the least particle of that metal.

the least acted upon, nor was any smell of nitric acid perceived; but on adding the smallest quantity of nitre to the same mixture, the gold was dissolved, and the smell of aqua regia was instantly perceived.*

A portion of the said bittern was next examined by appropriate re-agents, with a view to detect any minute quantity of earths or metals, especially alumina, silica, iron and copper, which former inquirers might have overlooked; but I could find no other earth except magnesia: and to my surprise, I did not find in the bittern the least particle of lime; which proves that sea-water contains no muriate of lime, as had been generally supposed. I was equally unsuccessful in my attempts to detect iron or copper, by the most delicate tests. In fact, neither alkalies, nor alkaline carbonates, precipitated any other substance from the bittern of sea-water, except magnesia.

The deposit obtained at Portsmouth during the early period of the concentration of the water, being analyzed, I found it to consist of selenite, mixed with a little muriate of soda, and a portion of carbonate of lime. The presence of this last subtance in sea-water, in a state of perfect solution, being, I believe, a new fact, I neglected no means of establishing it with certainty, an object which was accomplished without difficulty.†

Carbonate of magnesia having been supposed by some

* For this easy and elegant process for detecting nitric acid, a point attended with difficulty, I am indebted to Dr. Wollaston.

† The deposit was treated with acetic acid, which occasioned an effervescence. The clear liquor being then decanted off, and evaporated to dryness, and alcohol added, acetate of lime was found in the filtered alcoholic liquor.

chemists to exist in sea-water, I looked for it in the same deposit; but I could not detect the least portion of it by the most delicate tests.*

I next turned my attention to the alkaline salts of sea-water; and here I was more fortunate; as I succeeded in ascertaining beyond a doubt, that sea-water contains ammonia, as it yielded sal ammoniac by evaporation and sublimation. This result was easily obtained. Some of the bittern being evaporated to dryness in a retort, and a low red heat applied, a white sublimate appeared in the neck of the retort, which proved to be muriate of ammonia. The mode in which this substance was identified was as follows:

1. The sublimate was re-dissolved in water, re-evaporated to dryness, and again sublimed by the heat of a spirit lamp.

2. This new sublimate being again dissolved, and solution of magnesia and phosphoric acid added, a triple phosphate was formed.

3. On adding caustic potash to the solution, and bringing the mouth of a phial containing muriatic acid close to the vessel, abundant white fumes appeared.

4. The sublimate gave precipitates both with muriate of platina and nitrate of silver.†

Sulphate of soda having been mentioned by many chemists, as one of the constituents of sea-water, I endeavoured to ascertain, whether or not it existed in it. But all attempts

* Namely, solution of the mass in dilute muriatic acid; precipitation of the lime, and addition of phosphate of ammonia to the filtered liquor.

† As it did not enter into my plan, on this occasion, to turn my attention to the estimation of proportions or precise quantities, I have not attempted to estimate exactly the proportion which ammonia bears to the other ingredients of sea-water; but as its presence can easily be shown in 100 grains of the bitter salts, its quantity cannot be extremely minute.

to detect this salt in the bittern by crystallization were fruitless, though great pains were taken for that purpose; and I feel the more confident that there is no sulphate of soda in sea-water, as the presence of this salt, in any but the most minute quantities, would be quite incompatible with our knowledge of chemical affinities. For since there are, co-existing in sea-water, muriate of soda and sulphate of magnesia, it is evident that sulphate of soda would decompose muriate of magnesia, which salt is known to be in sea-water. And again we know, that sea-water contains sulphate of lime and muriate of soda; therefore it cannot contain sulphate of soda; for in that case we should have muriate of lime, which I have shown to be contrary to fact.

The last circumstance which I shall at present notice, relates to the state in which potash exists in sea-water.* Potash is found, by its appropriate re-agents, principally in the bittern; but it is found also among the salts which are separated from it, especially in the latter period of crystallization. By farther and repeated evaporation of the bittern, and successive separation of the mother water remaining after the removal of the crystals formed, various distinct crystals were obtained possessing their characteristic form, namely, prismatic sulphate of magnesia, cubic and star-shaped muriate of soda, and rhombic crystals, quite different from either of the other salts. These crystals, which were evidently portions of an oblique rhombic prism, being carefully separated and washed with water and alcohol, proved to be a triple salt of sulphate of

* It will be recollected, that the presence of potash in sea-water, though announced by myself in the paper on sea-water to which I have before alluded, was Dr. Wollaston's discovery. I have likewise to mention, that the above experiments respecting the state in which it exists, were either made by him or at his suggestion.

potash and magnesia; a salt so easily analyzed, that it would be quite superfluous to relate the particulars of the process.

It now remained to be ascertained, whether potash might not also exist in sea-water in the state of muriate of potash, or of triple muriate of potash and magnesia. That a considerable quantity of potash remains in the bittern, even after the separation of the triple sulphate, is easily ascertained; and by careful evaporation it may be made to crystallize as a triple salt in rhombic crystals; but the constitution of this salt is so delicate, that it is liable to be separated into muriate of potash and muriate of magnesia by water alone; and it is with certainty decomposed by alcohol, which takes up the magnesian muriate, and leaves the other undissolved.

From the foregoing observations and experiments it may, therefore, be inferred,

1st. That there is no mercury, or mercurial salt, in the waters of the ocean.

2dly. That sea-water contains no nitrates.

3dly. That it contains sal ammoniac.

4thly. That it holds carbonate of lime in solution.

5thly. That it contains no muriate of lime.

6thly. That it contains a triple sulphate of magnesia and potash.

Some of these circumstances will, of course, require that former analyses of sea-water, and my own in particular, should be corrected and revised; but this I shall not attempt to do, until I have obtained farther, and still more precise information on the subject.

Harley Street, 20 June, 1822.

XXXIV. *On the ultimate analysis of vegetable and animal substances.* By Andrew Ure, *M. D. F. R. S.*

Read June 27, 1822.

The improvements lately introduced into the analysis of vegetable and animal compounds, with the investigation of the equivalent ratios, in which their constituent elements, carbon, hydrogen, oxygen, and azote are associated, have thrown an unexpected light into this formerly obscure province of chemical science. While the substitution by M. Gay Lussac, of black oxide of copper for the chlorate of potash, has given peculiar facility and elegance to *animal* analysis, it may be doubted whether, in those cases, where the main object of inquiry is the proportion of carbon, it has not, frequently, led to fallacious results. As the quantity of this element is inferred from the volume of carbonic acid evolved in the decomposition of the organic matters, such of their particles as happen not to be in immediate contact with the cupreous oxide, will remain unconverted into carbonic acid; and thus the proportion of carbon will come to be underrated; an accident which cannot occur with chlorate of potash, since the carbonaceous matter is here plunged in an ignited atmosphere of oxygen. It is probably to this cause, that we must refer the discrepant results in the analysis of pure sugar, between M. M. Gay Lussac, Thenard, and Berzelius, on the one hand, and Dr. Prout, on the other; the former

gentlemen assigning about 43 parts in the hundred of carbon, while the latter states the carbon at only 40.*

The objects of the present paper are, first to indicate, and endeavour to remove several sources of fallacy attending the method with peroxide of copper; and next, to exhibit the results of its application to a considerable series of vegetable and animal compounds.

Peroxide of copper, prepared by igniting the pure nitrate of this metal, is, like yellow oxide of lead, and many other metallic oxides, readily absorbent of a small portion of humidity from the air, the quantity of which depends, in some measure, on the length of time during which it has suffered ignition. If exposed to a red heat, merely till the vapours of nitric acid are expelled, 100 grains of the oxide will absorb, in the ordinary state of the atmosphere, from one-tenth to two-tenths of a grain of moisture in the space of an hour or two; and about one half of the above quantity in a very few minutes. The French chemists, who have operated most with this agent, seem to be well aware of this circumstance, for they direct the peroxide to be used immediately after ignition, and to be triturated with the organic matter in a hot mortar of agate or glass. Yet this precaution will not entirely prevent the fallacy arising from the hygrometric action; for I find that peroxide thus treated does absorb, during the long trituration essential to the process, a certain quantity of moisture, which, if not taken into account, will produce serious errors in the analytical results. It is better therefore to leave the powdered peroxide intended for research, exposed for such time to the air, as to bring it to hygrometric repose,

* Dr. PROUT has informed me, since the above paper was written, that he re-triturated and re-ignited the contents of his tubes, in the analysis of sugar.

then to put it up in a phial, and by igniting one hundred grains of it in a proper glass tube, sealed at one end, and loosely closed with a glass plug at the other, to determine the proportion of moisture which it contains. This, then, indicates the constant quantity to be deducted from the loss of weight which the peroxide suffers in the course of the experiment. The mortar should be perfectly dry, but not warm.

Experimenters have been at great pains to bring the various organic objects of research to a state of thorough desiccation before mixing them with the peroxide of copper; but this practice introduces a similar fallacy to that above described. We ought, therefore, after having made them as dry as possible by the joint agencies of heat, and an absorbent surface of sulphuric acid in vacuo, to expose them to the air till they also come into hygrometric repose, noting the quantity of moisture which they imbibe, that it may be afterwards allowed for. The plan which I adopt for the purpose of desiccation seems to answer very well. Having put the pulverulent animal or vegetable matter into short phials, furnished with ground glass stoppers, I place the open phials in a large quantity of sand heated to 212° F. in a porcelain capsule, and set this over a surface of sulphuric acid in an exhausted receiver. After an hour or more the receiver is removed, and the phials instantly stopped. The loss of weight shows the total moisture which each of them has parted with; while the subsequent increase of their weight, after leaving them unstopped for some time in the open air, indicates the amount of hygrometric absorption. This is consequently the quantity to be deducted in calculating experimental results.

Many chemists, particularly in this country, have employed the heat of a spirit lamp, instead of that produced by the combustion of charcoal, for igniting the tube in which the mixed materials are placed. I have compared very carefully both methods of heating, and find that for many bodies, such as coal, and resin, which abound in carbon, the flame of the lamp is insufficient; while its application being confined at once to a small portion of the tube, that uniform ignition of the whole, desirable towards the close of the experiment, cannot be obtained. I was hence led to contrive a peculiar form of furnace, in which, with a handful of charcoal, reduced to bits about the size of small filberts, an experiment may be completed, without anxiety or trouble, in the space of half an hour. Since I have operated with this instrument, the results on the same body have been much more consistent, than those previously obtained with the lamp; and it is so convenient, that I have sometimes finished eight experiments in a day.

Fig. 1. (Plate L.) represents the whole apparatus, as when in action. Fig. 2 is a horizontal section of the furnace, in which we perceive a semi-cylinder of thin sheet iron, about 8 inches long and $3\frac{1}{2}$ wide, perforated with holes, and resting on the edge of a hollow prism of tin-plate, represented more distinctly in fig. 3, where *n* shows a slit, through which the sealed end of the glass tube may be made to project, on occasion. *i*, is a handle attached to the semi-cylinder, by which it may be slid backwards or forwards, and removed at the end of the process. *d*, is a sheath of platinum foil, which serves, by aid of a wire laid across, to support the middle of the tube, when it is softened by ignition. At *g*, the plates

which close the ends of the semi-cylinder and tin-plate prism, rise up a few inches to screen the pneumatic apparatus from the heat. A third occasional screen of tin-plate is hung on at *f*. All these are furnished with slits for the passage of the glass tube. This is made of crown glass, and is generally about 9 or 10 inches long, and $\frac{1}{10}$ of internal diameter. It is connected with the mercurial cistern by a narrow tube and caoutchouc collar. This tube has a syphon form, and rises about an inch within the graduated receiver at *e*. By this arrangement, should the collar be not absolutely air-tight, the pressure of the column of mercury causes the atmospheric air to enter at the crevice, and bubbles of it will be seen rising up without the application of heat. At the end of the operation, the point of the tube *e*, is always left above the surface of the mercury, the quantity of organic matter employed being such as to produce from 6 to 7 cubic inches of gaseous product, the volume of the graduated receiver being 7 cubic inches.

As the tubes with which I operate have all the same capacity, viz. half a cubic inch; and as the bulk of materials is the same in all the experiments, one experiment on the analysis of sugar or resin, gives the volume of atmospheric air due to the apparatus, which volume is a constant quantity in the same circumstances of ignition. And since the whole apparatus is always allowed to cool to the atmospheric temperature, the volume of residual gas in the tubes comes to be exactly known, being equal, very nearly, to the primitive volume of atmospheric air left after the absorption of the carbonic acid in the sugar or resin experiment.* Thus this

* If *a* be the capacity of the graduated receiver, and *b* the spare capacity of the tubes, then the above volume is $b - \frac{b}{a+b}$.

quantity, hitherto ill appreciated or neglected in many experiments, though it is of very great consequence, may be accurately found. At *k*, fig. 2, a little tin-plate screen is shown. It is perforated for the passage of the tube, and may be slid along, and left at any part of the semi-cylindric cage, so as to preserve from the influence of the heat, any requisite portion of the sealed end of the tube. At fig. 4 is seen the shape of the little bulb, into which I introduce the proper weight of ether, alcohol, naphtha, or other volatile liquids, which are destined for analysis. After weighing it exactly, it is immediately slid down to the bottom of the tube, and covered with 150 or 200 grains of peroxide of copper. The bulb has a capacity equal to 3 grain measures of water, and its capillary point is sometimes closed with an inappreciably small quantity of bees wax, to prevent the exhalation of the liquid, till the peroxide be ignited.

b is a cover to the furnace, with an oblong orifice at its top. It serves for a chimney, and may be applied or removed by means of its handle, according as we wish to increase or diminish the heat. *c c* are tin cases inclosing corks, through which the iron wires are passed, that support the whole furnace at any convenient height and angle of inclination.

The tightness of the apparatus at the end of the process, is proved by the rising of the mercury in the graduated receiver, by about one-tenth of an inch, as the tube becomes refrigerated.

My mode of operating with the peroxide of copper is the following:

I triturate very carefully in a dry glass mortar, from 1 to 2½ grains of the matter to be analyzed, with from 100 to 140

grains of the oxide. I then transfer it, by means of a platinum-foil tray and small glass funnel, into the glass tube, clearing out the mortar with a metallic brush. Over that mixture I put 20 or 30 grains of the peroxide itself, and next, 50 or 60 grains of clean copper filings. The remaining part of the tube is loosely closed with 10 or 12 grains of amianthus, by whose capillary attraction the moisture evolved in the experiment is rapidly withdrawn from the hot part of the tube, and the risk of its fracture thus completely obviated. The amianthus serves moreover as a plug, to prevent the projection of any minute particles of filings, or of oxide, when the filings are not present. The tube is now weighed in a very delicate balance, and its weight is written down. A little cork, channelled at its side, is next put into the tube, to prevent the chance of mercury being forced backwards into it, by any accidental cooling or condensation. The collar of caoutchouc is finally tied on, and the tube is placed, as is shown in fig. 2, but without the plate *k*, which is employed merely in the case of analyzing volatile liquids. A few fragments of ignited charcoal are now placed under the tube at the end of the furnace next to the cistern, and the remaining space in the semi-cylinder is filled up with bits of cold charcoal. The top, *b*, may then be put in its place, when the operation will proceed spontaneously, the progressive advance of the ignition from one end to the other being proportioned to the expansion of glass, so that the tube very seldom cracks in the process. Indeed I have often used the same tube for a dozen experiments, in the course of which it became converted into *vitrite*, or Reaumur's porcelain.

Since the evolved gas is saturated with moisture, I reduce

it to the volume of dry gas, by help of the following table, computed by the well known formula from my table of the elastic force of steam, which the Royal Society did me the honour to publish in their Transactions for the year 1818.

TABLE

Temperature.	Multiplier.	Temperature.	Multiplier.	Temperature.	Multiplier.
°		°		°	
53 F.	0.9870	61 F.	98.20	70 F.	0.9758
54	0.9864	62	98.13	71	0.9751
55	0.9858	63	98.06	72	0.9743
56	0.9852	64	97.99	78	0.9785
57	0.9846	65	97.93		
58	0.9839	66	97.86		
59	0.9833	67	97.79		
60	0.9827	68	97.72		
		69	97.65		

In certain cases, where the quantity of hydrogen is small, or where, as in the example of indigo, its presence has been denied, I employ pulverulent proto-chloride of mercury (calomel) instead of peroxide of copper. The organic compound being intimately mixed with that powder, and gently heated, the muriatic acid gas obtained, demonstrates the presence, though half of its volume will not give the total quantity of, hydrogen; for a proportion of this elementary body continues associated with oxygen in the state of water. Dry oxalate of lead, treated in this way, yields not the slightest trace of muriatic acid, for on passing the disengaged gas through a dilute solution of nitrate of silver, no precipitation or

even cloud of chloride is produced. But 5 grains of indigo, prepared from the de-oxidized solution of the dyer's vat, and freed from its lime and resin by the successive application of dilute muriatic acid and alcohol, gave 5 cubic inches of muriatic acid gas when heated along with 150 grains of calomel. Here we have a quantity of gas equivalent to $2\frac{1}{2}$ cubic inches of hydrogen. By means of peroxide of copper, however, nearly 4 times the above quantity of hydrogen may be obtained from the same weight of indigo.

I shall now give in detail, one example of the mode of computing the relation of the constituents from the experimental results, and shall then state the other analyses in a tabular form, subjoining a few remarks on the habitudes of some peculiar bodies.

1.4 grains of sulphuric ether, specific gravity 0.70, being slowly passed in vapour from the glass bulb through 200 grains of ignited peroxide of copper, yielded 6.8 cubic inches of carbonic acid gas at 66° F. which are equivalent to 6.57128 of dry gas at 60°. This number being multiplied by 0.127 = the carbon in 1 cubic inch of the gas, the product 0.8345256, is the carbon in 1.4 grains of ether; and $0.8345256 \times \frac{1}{3} = 2.2254$ = the oxygen equivalent to the carbonic acid. The tube was found to have lost 4.78 grains in weight, 0.1 of which was due to the hygrometric moisture in the oxide, and 1.4 to the ether. The remainder, 3.28, is the quantity of oxygen abstracted from the oxide by the combustible elements of the ether. But of these 3.28 grains, 2.2254 went to the formation of the carbonic acid, leaving 1.0546 of oxygen, equivalent to 0.1318 of hydrogen. Hence, 1.4 ether, by this experiment, which is taken as the most satisfactory of

a great number, seem to consist of

Carbon	0.8345
Hydrogen	0.1318
Water	0.4337
	1.4000

And in 1 grain we shall have

Carbon	0.5960	8 atoms	2.25	60.00
Hydrogen	0.1830	4 atoms	0.50	13.33
Oxygen	0.2710	1 atom	1.00	26.66
	1.0000		3.75	100.0

Or, 8 volumes olefiant gas $= 8 \times 0.9722 = 2.9166$

2 . . vapour of water . $2 \times 0.625 = 1.25$

4.1666

which suffering a condensation equal to the whole vapour of water, will give an ethereous vapour, whose specific gravity is 2.5.

The proportion of the constituents of sulphuric ether, deduced by M. GAY LUSSAC from the experiments of M. TH. DE SAUSSURE, are 2 volumes olefiant gas + 1 volume vapour of water, which 8 volumes are condensed into 1 of vapour of ether, having a specific gravity = 2.58. The ether which I used had been first distilled off dry carbonate of potash, and then digested on dry muriate of lime, from which it was simply decanted, according to the injunction of M. DE SAUSSURE. Whether my ether contained more aqueous matter than that employed by the Genevese philosopher, or whether the difference of result is to be ascribed to the difference in the mode of analysis, must be decided by future researches.

By analogous modes of reduction, the following results were deduced from my experiments. I ought here to state, that in many cases the materials, after being ignited in the tube, and then cooled, were again triturated in the mortar, and subjected to a second ignition. Thus, none of the carbon could escape conversion into carbonic acid. I was seldom content with one experiment on a body; frequently six or eight were made.

Table of organic analyses.

	Substance.	Carbon.	Hydrogen.	Oxygen.	Azote.	Water.	Excess.
1	Sugar	43.38	6.29	50.33		56.62	
							Oxygen
2	Sugar of diabetes	39.52	5.57	54.91		51.13	10.35
3	Starch	38.55	6.13	55.32		55.16	6. 3
4	Gum Arabic	35.13	6.08	55.79	3?	54.72	7.15
							Hydrogen
5	Resin	73.60	12.90	13.50		15.20	11.20
6	Copal	79.87	9.00	11.10		12. 5	7. 6
7	Shell lac	64.67	8.22	27.11		30.51	4.82
8	Resin of guaiac.	67.88	7.05	25.07		28.	3.93
9	Amber	70.68	11.62	17.77		20. 0	9.40
10	Yellow wax	80.69	11.37	7.94		8.93	10.39
11	Caoutchouc	90.00	9.11	0.88		.99	9.00
12	Splent coal	70.90	4.30	24.80		27.90	1.20
13	Cannel coal	72.22	3.93	21.05	2. 8	23.68	1.30
14	Indigo	71.37	4.38	14.25	10.	16. 0	2.52
15	Camphor	77.38	11.14	11.48		12.91	9.71
16	Naphthaline	91. 6	7. 7	0.70?		0.79?	
17	Spermaceti oil	78.91	10.97	10.12		11.34	9.71
18	Common oil of turpentine	82.51	9.62	7.87		8.85	8.64
19	Purified oil of turpentine	84. 9	11. 5	3. 6		4. 0	11. 1
20	Naphtha	83.04	12.31	4.65		5.23	11.73
21	Asiatic castor oil	74.00	10.29	15.71		17.67	8.33
22	Alcohol, sp. gr. 0.812	47.85	12.24	39.91		44. 9	7.25
23	Ether, specific gravity 0.70	59.60	13. 3	27. 1		30. 5	9. 9
							Oxygen
24	Bleached Silk	50.69	3.94	34.04	11.33	35.43	2.55
25	Cotton	42.11	5.06	52.83		45.56	12.33
26	Flax, by Lee's process	42.81	5. 5	51. 7		49. 5	7. 7
27	Common flax	40.74	5.57	52.79	0. 9	50.16	8. 2
28	Wool	53. 7	2.80	31. 2	12. 3	25. 7	8. 3
							Hydrogen
29	Cochineal	50.75	5.81	36.53	6.91	39. 6	14. 1
30	Captharides	48.64	5.99	36.29	9.08	40.83	14.53
31	Urea	18.57	5.93	43.68	31.82	49.14	0.47
32	Benzoic acid	66.74	4.94	28.32		31.86	1. 4
							Oxygen
33	Citric acid	33.00	4.63	62.37		41.67	25.33
34	Tartaric acid	31.42	2.76	65.82		24.84	43.74
35	Oxalic acid	19.13	4.76	76.20		42.87	38.09
36	Ferroprussic acid	36.82	27.89 of iron.		35.29		

Remarks on the preceding analyses.

1. Sugar. The sugar which I employed had been purified by Mr. HOWARD's steam process, and was so well stove-dried, that it lost no appreciable portion of its weight, when enclosed along with sulphuric acid in *vacuo*. The diabetic sugar has a manifest excess of oxygen, which I believe to be the case with all weak sugars, as they are called by the sugar refiners. I consider this excess of oxygen as the chief cause which counteracts crystallization, and therefore the great obstacle to the manufacturer. The smallest proportion of carbon, which I have ever found in any cane sugar, was upwards of 41 per cent. The experiments on starch and gum were among the earliest which I made, and the results differ so much from those given by other experimenters, that I shall repeat the analyses at the earliest opportunity. The constituents of the above three bodies, referred to the prime equivalent scale, will be approximately as follows:

Sugar.			Starch.			Gum.		
Carbon 5 atoms	3.75	45.4	5 atoms	3.75	40.5	4 atoms	3.0	35.25
Oxygen 4 ——	4.00	48.5	5 ——	5.00	54.0	5 ——	5.0	58.90
Hydrogen 4 ——	0.50	6.1	4 ——	0.50	5.5	4 ——	0.5	5.85
	8.25	100.0		9.25	100.0		8.5	100.00

I conceive the purest and strongest sugar to be constituted as here represented.

All the elementary principles of organic nature may be considered as deriving the peculiar delicacy of their chemical equilibrium, and the consequent facility with which it may be subverted and new modelled, to the multitude of atoms

grouped together in a compound. On this view, none of them should be expected to consist of a single atom of each component. The allurement of theoretic simplicity has led some ingenious philosophers to represent sugar by 1 atom of carbon, 1 atom of oxygen, and 1 atom of hydrogen; or of 40 carbon, 53⅓ oxygen, and 6⅔ hydrogen in 100 parts. But I am satisfied that all sound specimens of sugar will yield considerably more carbon than 40 *per cent.*

Starch is liable to a similar deterioration with sugar; that is, some species of it make a much firmer coagulum with hot water than others; a difference probably due to the proportion of oxygen. The starch here employed was that of commerce, and was not chemically desiccated: hence, the redundancy of water beyond the equivalent proportion. A little hygrometric moisture was present also in the gum, as it was not artificially dried. A note of interrogation is placed after azote. That doubt will I trust be solved, when I complete my analyses of grains, roots, and leaves, with the view of tracing the origin of azote in the bodies of graminivorous animals.

We now come to a class of bodies in which the hydrogen predominates over the oxygen. With regard to resin, I believe the quantity of its carbon to be somewhat underrated in the table. Though three experiments were made on it, I now perceive that I had omitted to re-triturate and re-ignite; and the carbon of resin is very difficult of oxygenation. Its true composition is probably,

Carbon	8 atoms	6.0	75.00
Hydrogen	8 ——	1.0	12.50
Oxygen	1 ——	1.0	12.50
		8.0	100.00

A still more symmetric arrangement would be derived from

Carbon	8 atoms	6.0	73.9
Hydrogen	9 ——	1.125	13.8
Oxygen	1 ——	1.000	12.3
		8.125	100.0

This proportion corresponds to 8 atoms of olefiant gas and 1 atom of water; and I think it is very possibly the true constitution of resin. Had the loss of weight suffered by the contents of the tube, during their ignition, been a few hundredth parts of a grain more, the experimental result would have coincided with this theoretical view. Copal approaches to

Carbon	10 atoms	7.5	80.80
Hydrogen	7 ——	0.875	9.36
Oxygen	1 ——	1.000	10.84
		9.375	100.00

Lac may be nearly represented by

Carbon	6 atoms	4.5	64.3
Hydrogen	4 ——	0.5	28.5
Oxygen	2 ——	2.0	7.2
		7.0	100.0

or 2 atoms of olefiant gas + 1 atom carbonic oxide; that is equal weights of these two binary compounds; for

2 atoms of olefiant gas $= 2 \times (0.75 + 0.125) = 1.75$
1 atom carbonic oxide . . . $= 1.75$

Referred to volumes, we shall have lac to consist of equal parts of the two gases.

Resin of guaiac. gives

Carbon	7 atoms	5.25	67.7
Hydrogen	4 ——	0.50	6.5
Oxygen	2 ——	2.00	25.8
		7.75	100.0

Although the experiments on amber were conducted carefully with re-trituration and re-ignition, no good atomic configuration of it has occurred to me. It approaches to 10 carbon + 10 hydrogen 10 hydrogen + 2 oxygen.

Wax is apparently composed of

Carbon	18 atoms	9.75	80.4
Hydrogen	11 ——	1.375	11.3
Oxygen	1 ——	1.000	8.3
		12.125	100.0

or in other words, of 11 atoms olefiant gas + 1 atom carbonic oxide + 1 atom carbon. Had the experiment given a very little more hydrogen, we should have had wax as consisting of 12 atoms olefiant gas + 1 atom carbonic oxide. This is possibly the true constitution.

Caoutchouc seems to consist of

Carbon	3 atoms	2.25	90
Hydrogen	2 ——	0.25	10
		2.50	100

Or it is a sesqui-carburetted hydrogen. The oxygen deduced from experiment is in such small quantity, as to leave a doubt whether it be essential to this body, or imbibed in minute quantity from the air during its consolidation.

Splent or slate coal, specific gravity 1.266, abstracting its incombustible ashes, approaches in constitution, to

Carbon	7 atoms	5.25	70.00
Hydrogen	3 ——	0.375	3.40
Oxygen	2 ——	2.000	26. 6
		7.625	100.0

Cannel coal from Woodhall, near Glasgow, specific gravity 1.228, resembles a compound of

Carbon	9 atoms	6.750	73.9
Hydrogen	8 ——	0.375	4.2
Oxygen	2 ——	2.000	21.9
		9.125	100.0

In both of these bodies there is an excess of carbon beyond the 3 atoms of olefiant gas and 2 of carbonic oxide. The former coal has 2 extra atoms of carbon, and the latter 4 atoms. Hence, this coal is found at the Glasgow gas works to yield a very rich burning gas. I do not know whether the azote be essential to the constitution of this coal, or accidentally introduced from animal remains at the formation of the strata.

The elements of indigo may be grouped as follows:

Carbon	16 atoms	12.0	72.70
Hydrogen	6 ——	0.75	4.55
Oxygen	2 ——	2.00	12.15
Azote	1 ——	1.75	10.60
		16.50	100.00

or, in other terms, we shall have

1 atom cyanogen, 6 atoms olefiant gas, 2 atoms carbonic oxide, and 6 atoms of carbon in excess.

I had intended to pursue, at considerable detail, my researches on this curious azotized product of vegetation, but the subject having been lately taken up, and ingeniously prosecuted by my pupil and friend, Mr. WALTER CRUM, I was induced to leave it in his hands. He announced to me the presence of hydrogen in indigo, before I had analyzed this substance myself; and drew my attention, particularly, to

the fallacy occasioned by the hygrometric water of the peroxide of copper. It is likely that some slight modification may require to be made in my tabular proportion of the constituents, for I did not resume the subject of indigo, after I had become most familiar with the manipulations.

Camphor is very nearly represented by

Carbon	10 atoms	7.5	78.02
Hydrogen	9 ——	1.125	11.58
Oxygen	1 ——	1.0	10.40
		9.625	100.00

or 9 atoms olefiant gas + 1 atom carbonic oxide. Naphthaline is, in my opinion, a solid bi-carburet of hydrogen, consisting of

Carbon	2 atoms	1.5	92.9
Hydrogen	1 ——	0.125	7.1
		1.625	100.0

It is very difficult, even by the best regulated ignition, to resolve the whole carbon of this very volatile body into carbonic acid; hence, the carbon may come to be underrated in the result. Naphthaline is obtained during the rectification of the petroleum of the coal gas works. It is found encrusting the pipes in the form of a greyish crystalline mass; and when purified by a second sublimation at the temperature of about 220°, it forms beautiful thin plates, white and glistening. It has a powerful petroleum odour. With brine of the specific gravity 1.048, these plates, when once thoroughly wetted (which is difficult to effect) remain in equilibrium; that is, float in any part of the liquid. That number, therefore, represents the specific gravity of naphthaline. It is insoluble in water, but very soluble in ether, and moderately so in alcohol. With iodine, it fuses at a gentle

heat into a brown liquid, forming as it cools a solid resembling plumbago, which dissolves readily in alcohol, and is thrown down by water. Naphthaline is soluble in oils. In water heated to 168° F. it fuses, and remains like oil at the bottom of the liquid; but when stirred it rises, and spreads on the top in little oily patches. At 180° it rises spontaneously from the bottom in oily globules, which, as the temperature is raised, dissipate in the air, undergoing motions similar to those of camphor floating on water.

Spermaceti oil is constituted apparently of

Carbon	10 atoms	7.5	78.0
Hydrogen	9 ——	1.125	11.8
Oxygen	1 ——	1.0	10.2
		9.625	100.0

or, in other words, of 9 atoms olefiant gas + 1 atom carbonic oxide. The experimental proportion is, however, more nearly,

Carbon	10 atoms	7.5	79.0
Hydrogen	8 ——	1.0	10.5
Oxygen	1 ——	1.0	10.5
		9.5	100.0

There is here an atom of carbon in excess.*

Common oil of turpentine, specific gravity 0.888, comes very closely to the following arrangement:

Carbon	14 atoms	10.5	82.35
Hydrogen	10 ——	1.25	9.80
Oxygen	1 ——	1.00	7.85
		12.75	100.00

* This is probably the truer view. The former would make it coincide with camphor.

Oil of turpentine, purified with alcohol by Dr. Nimmo's method, seems to approach to the constitution of naphtha, or of a mere carburet of hydrogen. Its specific gravity is 0.878. But as from the mode of preparing it, a minute portion of alcohol may remain in it, I do not think it necessary to investigate its atomical structure.

Naphtha, specific gravity 0.857, obtained by distillation from petroleum, is very nearly represented by,

Carbon	22 atoms	16.50	82.5
Hydrogen	20 ——	2.50	12.5
Oxygen	1 ——	1.00	5.0
		20.00	100.0

It therefore consists of 20 atoms olefiant gas, 1 atom carbonic oxide, and 1 atom of carbon held in solution.

Castor oil is an interesting unctuous body, from its great solubility in alcohol. It consists nearly of

Carbon	17 atoms	5.25	75.00
Hydrogen	6 ——	0.75	10.70
Oxygen	1 ——	1.00	14.30
		7.00	100.00

It is composed therefore of 6 atoms olefiant gas + 1 atom carbonic oxide; or in volumes of 3 olefiant gas + 1 carbonic oxide.

Alcohol, specific gravity 0.812, is composed very nearly of

Carbon	8 atoms	2.250	46.15
Hydrogen	5 ——	0.625	12.82
Oxygen	2 ——	2.000	40.03
		4.875	100.00

or, of 8 atoms olefiant gas = 2.625.
2 —— water = 2.25.

And in volumes, 3 olefiant gas $= .9729 \times 3 = 2.9168$
4 aqueous vapour $= .625 \times 4 = 2.500$

Thus alcohol of 0.812, by the above analysis, which I believe merits confidence, from the care and consistency of the experiments, differs from M. Gay Lussac's view of absolute alcohol, deduced from M. Th. de Saussure's experiments, in containing an additional volume of aqueous vapour. At the specific gravity .814 alcohol would have exactly this atomic constitution. If the condensation be equal to the whole 3 volumes of olefiant gas, that is if the 7 volumes of constituent gases become 4 of alcohol vapour, we shall have its specific gravity at this strength $= 1.3729$; the additional volume of aqueous vapour producing necessarily this abatement in the density.

We have already considered sulphuric ether.

The next 4 bodies which occur in the table are of fibrous texture, and are chemically characterized by an excess of oxygen beyond the hydrogen equivalent.

Fibres of the bleached threads of the silk-worm were subjected to analysis. Their composition is apparently,

Carbon	10 atoms	. .	7.5	. .	50.8
Hydrogen	4 ——	. .	0.5	. .	3.4
Oxygen	5 ——	. .	5.0	. .	34.0
Azote .	1 ——	. .	1.75	. .	11.8
			14.75		100.0

or, 4 of olefiant gas, 5 of carbonic oxide, and 1 of nitrous oxide; or of 1 atom prussic acid, 3 atoms olefiant gas, and 5 atoms carbonic oxide.

Cotton fibres, unbleached, seem to consist of

Carbon	11 atoms	8.25	42.85
Hydrogen	8 ——	1.00	5.30
Oxygen	10 ——	10.00	51.85
		19.25	100.00

Flax, by LEE's patent process, consists of

Carbon	7 atoms	5.25	44.25
Hydrogen	5 ——	0.625	5.25
Oxygen	6 ——	6.000	50.50
		11.875	100.00

It contains more carbon, and is therefore probably stronger than common flax, prepared by a putrefactive maceration. This seems composed of

Carbon	1 atom	0.75	40.0
Hydrogen	1 ——	0.125	6.6
Oxygen	1 ——	1.000	53.4
		1.875	100.0

But this is the theoretical representation of sugar by M. GAY LUSSAC and Dr. PROUT; and, hence, these chemists would readily explain, how linen rags may pass into the form of sugar, by the action of sulphuric acid.

Wool approximates to

Carbon	10 atoms	7.5	55.0
Hydrogen	3 ——	0.375	2.8
Oxygen	4 ——	4.000	29.4
Azote	1 ——	1.750	12.8
		13.625	100.0

Cochineal seems to be made up of

Carbon	15 atoms	11.250	50.20
Hydrogen	11 ——	1.375	6.15
Oxygen	8 ——	8.000	35.85
Azote	1 ——	1.750	7.80
		22.375	100.00

Cantharides approximate to

Carbon	11 atoms	9.75	49.4
Hydrogen	10 ——	1.25	6.8
Oxygen	7 ——	7.00	35.4
Azote	1 ——	1.75	8.9
		19.75	100.0

My result with urea differs so considerably in the proportion of azote, from that of Dr. PROUT and M. BERARD, that I am disposed to doubt of the accuracy of my experiments, though they were made with the utmost care, and were most consistent in the repetition. I could perceive no smell whatever of nitrous gas in the gaseous products, which were made to traverse a column of copper filings 8 inches long, in a state of ignition. I shall renew the inquiry on urea, and employ the lowest temperature compatible with the formation of carbonic acid.

The prime equivalent of benzoic acid crystals, I find by saturation with water of ammonia, to be 14.5; and it consists apparently of

Carbon	13 atoms	9.75	67.24
Hydrogen	6 ——	0.75	5.16
Oxygen	4 ——	4.00	27.60
		14.50	100.00

Of crystalline citric acid, the prime equivalent is 8.375 by my experiments; and it consists probably of

Carbon	4 atoms	3.000	35.8
Hydrogen	3 ——	0.375	4.5
Oxygen	5 ——	5.000	59.7
		8.375	100.0

or, of 4 atoms carbon, 3 water, and 2 oxygen. 2 of these atoms of water are separated, when citric acid is combined with oxide of lead in what is called the dry citrate. Hence, the acid atom is in this case 6.125.

The prime equivalent of crystalline tartaric acid is 9.25 by my results; and it seems made up of

Carbon	4 atoms	3.0	32.43
Hydrogen	2 ——	0.25	2.70
Oxygen	6 ——	6.00	64.87
		9.25	100.00
or, of Carbon	4 atoms	3.0	32.43
Oxygen	4 ——	4.0	43.24
Water	2 ——	2.25	24.33
		9.25	100.00

From my experiments I have been led to conclude, that into dry tartrate of lead these two atoms of water *do* enter as a constituent; and, hence, that the crystals of tartaric acid are as dry as is compatible with its constitution.

Oxalic acid crystals have 7.875 for their prime equivalent; and are composed of

Carbon	2 atoms	1.500	19.04
Hydrogen	3 ——	0.375	4.80
Oxygen	6 ——	6.000	76.16
		7.875	100.00

or, of 2 atoms Carbon	1.5	19.14
3 —— Oxygen	3.0	40.72
3 —— Water	3.875	40.14
	7.875	100.00

Into the dry oxalate of lead these 3 atoms of water do *not* enter. Hence, I find the dry acid to be composed of

Carbon 2 atoms	1.5	33.33
Oxygen 3 ——	3.0	66.66
	4.5	100.00

or, of 1 atom carbonic acid + 1 atom carbonic oxide, as was first suggested, I believe, by DOBEREINER. Crystallized oxalate of ammonia consists of 1 atom acid, 1 atom ammonia, and 2 atoms water = 8.875. By a gentle heat 1 atom of water may be separated; and an oxalate of ammonia, as dry as is compatible with its neutrality, remains.

I have analyzed, by the peroxide of copper, the citrate, tartrate, and oxalate of lead; and on comparing the results thus obtained, with those derived from the analysis of the crystalline acids, I have come to the above determinations.

Ferroprussic acid, the ferrocyanic acid of the French chemists, has proved, hitherto, a stumbling block to me, in reducing the results of my experiments to the atomic theory. I have subjected it to very numerous trials in many states of combination, and have sought, with great pains, to accommodate the results to the doctrine of prime equivalents; but hitherto without success. The following facts, however, may perhaps be deemed of some consequence.

In the first place, the prime equivalent of the crystallized ferroprussiate of potash is 13.125, compared to oxide of lead

14, and to nitrate of the same metal 20.75; that is, 13.195 of the former salt neutralize 20.75 of the latter. In the second place, 14 parts of oxide of lead yield 21 parts of dry ferroprussiate of lead; or the atomic weight of dry ferroprussic acid is 7.

The mean of my analyses of ferroprussiate of lead, gives the relation of the constituents of the acid, as marked in the table. These proportions, reduced to the atomic weight 7, afford

Carbon	2.5774
Azote	2.4708
Ferreous matter	1.9518
	7.0000

Were we to suppose the prime equivalent of the ferroprussic acid 7.5 instead of 7; and were we farther to suppose that the carbon in the above result should be $2.25 = 3$ atoms, and the azote $= 3.5$, or two atoms, then we might conceive an atom of dry ferroprussic acid to be made up of

Carbon	3 atoms		2.25
Azote	2	——	3.50
Iron	1	——	1.75
			7.50

But experiment does not permit me to adopt this theoretical representation.

The best mode that has occurred to me for analyzing ferroprussiate of potash, is to convert it, by the equivalent quantity of nitrate of lead, into the ferroprussiate of this metal; then to separate the nitrate of potash by filtration; and, after evaporation, to determine its weight. In this way,

13.125 grains of crystallized ferroprussiate of potash afford 12.33 grains of nitre, which contain 5.8 of potash.* By heating nitric acid in excess on 21 grains of ferroprussiate of lead, I obtained 2.625 grains of peroxide of iron, equivalent to 1.8375 of the metal. Hence I infer, that the iron in the ferroprussiate of lead is in the metallic state; for the joint weights of the carbon and azote contained in 7 grains of the dry acid is 5.0477; and the difference, 1.9523, approaches too closely to the above quantity, 1.8375, for us to suppose the metal to be in the state of protoxide. In fact, 2.625 parts of peroxide $\times 0.9 = 2.3625$ of protoxide, is a quantity much beyond what experiment shows to be present.

* By careful desiccation, 1.69 grains of water, may be separated from 13.125 grains of the salt.

Horizontal Section.

Fig. 2.

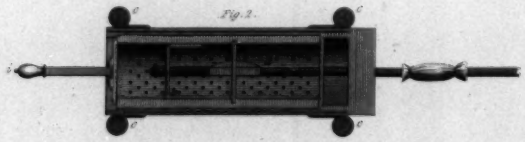


Fig. 3

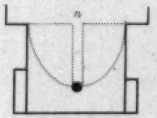
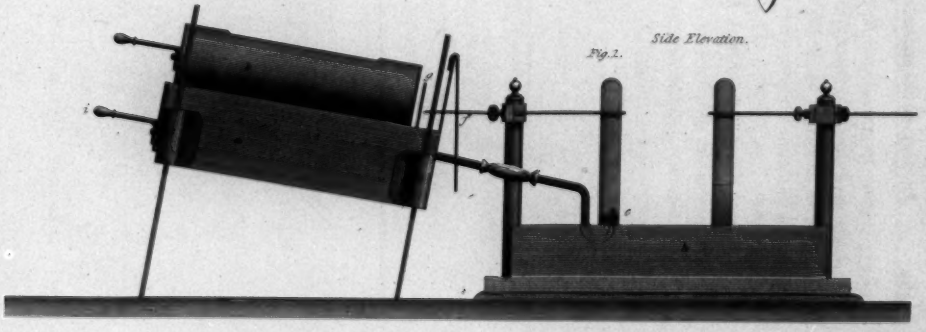


Fig. 4.

Fig. 2.

Side Elevation.



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WITH THE

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1821.	PRESENTS.	DONORS.
Nov. 8.	The Statutes of the Realm, printed by Command of H. M. King George the III. in pursuance of an Address of the House of Commons of Great Britain. Vol. VIII. 1821. F°	The Commissioners of Public Records.
	The Asiatic Researches, or Transactions of the Society instituted in Bengal, for inquiring into the History and Antiquities, the Arts, Sciences, and Literature, of Asia. Vol. XIII. Calcutta, 1820. 4°	The Asiatic Society.
	A Dictionary of the Chinese Language. Vol. I. Part 4. by R. Morrison, D.D. Macao, 1820. 4°	The Court of Directors of the Honourable East India Company.
	Mémoires de l'Académie Imperiale des Sciences de St. Petersbourg. Vols. V. VI. and VII. St. Petersburgh, 1815, 1818, 1820. 4°	The Imperial Academy of St. Petersburgh.
	The Universal Cambist and Commercial Instructor, being a full and accurate Treatise on the Exchanges, Monies, Weights and Measures of all trading Nations and their Colonies, with an Account of their Banks, Public Funds, and Paper Currencies, by P. Kelly, LL. D. 2d Edition. London, 1821. 2 Vols. 4°	Dr. P. Kelly.
	An Account of the means which have been taken to preserve the British Navy, from the earliest period to the present time, particularly from that species of Decay now denominated Dry Rot, by John Knowles. London, 1821. 4°	John Knowles, Esq.
	Memoirs of the American Academy of Arts and Sciences. Vol. IV. Part 2. Cambridge, 1821. 4°	The American Academy of Arts and Sciences.
	Mémoire sur la Distribution de la Chaleur dans les Corps solides. 4°	M. Poisson.

1821.	PRESENTS.	DONORS.
Nov. 8.	Medico-Chirurgical Transactions, published by the Medical and Chirurgical Society of London. Vol. XI. part 2. London, 1821. 8°	The Medical and Chirurgical Society of London.
	The Principles and Doctrine of Assurances, Annuities on Lives, and Contingent Reversions, stated and explained, by William Morgan, F.R.S. London, 1821. 8°	William Morgan, Esq.
	A Compendious Treatise on the Theory and Solution of Cubic and Biquadratic Equations, and of Equations of the higher order, by the Rev. B. Bridge, B.D. F.R.S. London, 1821. 8°	Rev. B. Bridge.
	The proximate Causes of material Phænomena, and the true Principles of universal Causation considered and illustrated, by Sir Richard Phillips. London, 1821. 12°	Sir Richard Phillips.
	A Collection of Tracts. 8°	Dr. Andrew Duncan.
	The Nautical Almanac, and Astronomical Ephemeris for the year 1824.	The Commissioners of Longitude.
	The Works of Confucius, in Chinese. 5 Parts. 8°	Mr. William Huttman.
	Voyages dans la Grande Bretagne entrepris relativement aux Services publics de la Guerre et de la Marine, et des Ponts et Chaussées, en 1816-1819, par Ch. Dupin. Paris, 1820. 4 Vols. 4°	M. Dupin.
15.	Flora Batava. No. 59.	H.M. The King of the Netherlands.
	The Modern Church, a Satirical Poem, comprising Sketches of some popular and unpopular Preachers, by John Laurens Bicknell, F.A.S. London, 1820. 8°	John Laurens Bicknell, Esq.
22.	Vetus Testamentum Græcum a Codice MS. Alexandrino qui Londini in Bibliotheca Musei Britannici asservatur Typis ad similitudinem ipsius Codicis Scripturæ fideliter descriptum curâ et labore H. H. Baber, A.M. Vol. I. London, 1816. f°	The Trustees of the British Museum.
	A Natural History of the Crinoeidea or Lilly-shaped Animals, with Observations on the Genera Asteria, Euryale, Comatula and Marsupites, by J. S. Miller. Bristol, 1821. 4°	Mr. J. S. Miller.
	An Account of the Animals seen by the late Northern Expedition, whilst within the Arctic Circle, by Captain Edward Sabine, R.A. London, 1821. 4°	Captain Edward Sabine.
	Ragguaglio del Reale Osservatorio di Napoli eretto sulla Collina di Capodimonte 4°	M. Piazzi.
	Ephemeris of the Distances of the four Planets, Venus, Mars, Jupiter, and Saturn, from the Moon's Centre, for 1822, to which is annexed, Tables for finding the Latitude of the Polar Star for 1821-1822, by H. C. Schumacker. Copenhagen, August, 1820. 4°	Professor Schumacker.
	Astronomische Hülfstafeln für 1821, von H. C. Schumacker. Copenhagen. 8°	

1821.	PRESENTS.	DONORS.
Nov. 22.	An additional Plate and Sheet to the History of the Soke of Grantham.	Edmund Turnor, Esq.
Dec. 6.	Transactions of the Geological Society, Vol. V. Part 2. London, 1821. 4°	The Geological Society.
	Annales des Mines ou Recueil des Mémoires sur l'Exploitation des Mines et sur les Sciences qui s'y rapportent, redigées par le Conseil Général des Mines. No. 2 and 3 for 1821. 8°	Le Conseil Général des Mines.
	Of Population. An Inquiry concerning the Power of Increase in the Numbers of Mankind, being an Answer to Mr. Malthus' Treatise on that Subject, by W. Godwin. London, 1820. 8°	William Godwin, Esq.
	The Introductory Lecture of a Course upon State Medicine, by John Elliotson, M. D. London, 1821. 8°	John Elliotson, M. D.
	Lezioni Elementari di Cosmografia, di Geografia, e di Statica di Jacopo Gråberg di Hemso. Genoa, 1819. 8°.	M. J. Gråberg di Hemso.
	Lettera del Sig Gråberg di Hemso all' Illus. Sig. L. Gropi sulla Peste di Tangeri negli anni 1818-19. Genoa, 1820. 8°	———
1822.		
Jan. 10.	Histoire Naturelle et Medicale des Casses, et particulièrement de la Casse et des Sénés employés en Medécine par L. T. F. Colladon, M. D. Montpellier, 1816. 4°	Dr. Colladon.
	Narrative of a Descent in the Diving Bell, by Dr. Louis Theodore Frederick Colladon. 8°	———
	Introduction to a synoptical Table of English and French Lineal Measures. 4°	Mr. Goodwyn.
	Astronomical Tables and Remarks for the Year 1822, by Francis Baily, F. R. S. London, 1822. 8°	Francis Baily, Esq.
	Meteorological Journal kept on board the Marine Society's Ship in the Year 1821.	The Committee of Governors of the Marine Society.
	Poesias Lyricas de Francisco de Borja Garçao Stockler. Londres, 1821. 8°	M. F. B. G. Stockler.
	Observations et Reflexions concernant la Polarité de la Chaleur. 4°	M. Henri de Schuzler.
17.	A Collection of Examples of the Application of the Differential and Integral Calculus, by George Peacock. Cambridge, 1820. 8°	Mr. Peacock.
	A Collection of the Examples of the Applications of the Calculus of Finite Differences, by J. F. W. Herschel. Cambridge, 1820. 8°	J. F. W. Herschel, Esq.
24.	Memoir of the late Captain Joseph Huddart, F. R. S. London, 1821. 4°	Sir Joseph Huddart.
	Chart of the Southern part of the China Sea. And Chart of the Straits and Islands at the South-west part of the China Sea.	James Horsburgh, Esq.
31.	Calendarium Inquisitionum post Mortem sive Escætarum. Vol. III. Temp. Ric. 2 et Hen. 4, 1821. F°	The Commissioners of Public Records.

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Jan. 31.	Valor Ecclesiasticus Temp. Henr. VIII. Auctoritate Regiâ institutus. Vol. IV. 1821. F°	The Commissioners of Public Records.
	An Introductory Discourse delivered to the Leeds Philosophical and Literary Society, April 6, 1821, by Charles Turner Thackrah. Leeds. 4°	C. T. Thackrah, Esq.
	Transactions of the Society Instituted at London for the Encouragement of Arts, Manufactures, and Commerce. Vol. XXXIX. London, 1821. 8°	The Society of Arts.
Feb. 7.	Mémoire sur la Combinaison de l'Oxigène avec l'Eau et sur les Propriétés extraordinaires que possède l'Eau oxigenée, par M. Thenard. 4°	M. Thenard.
	Observations sur la Physiologie Végétale et sur la Système physiologique de M. Aubert du Petit Thouars, par M. Feburier. 8°	M. Feburier.
	Rapport sur les Moyens proposés jusqu'à ce jour pour préserver les Blés de la Carie, par M. Feburier. 8°	———
	The Chemical Catechism, with Tables, Notes, Illustrations, and Experiments, by Samuel Parkes. London, 1822. 8°	Samuel Parkes, Esq.
21.	Memoirs of the Board of Agriculture of the State of New York. Vol. I. Albany, 1821. 8°	Dr. David Hosac.
	Narrative Journal of Travels through the Northwest Regions of the United States, extending from Detroit through the great Chain of American Lakes to the Sources of the Mississippi River, performed as a Member of the Expedition under Governor Cass in the year 1820, by Henry R. Schoolcraft. Albany, 1821, 8°	———
	Observations Physico-Chimiques sur les Alliages du Potassium et du Sodium avec d'autres Métaux, Propriétés de ces Alliages, &c. par G. S. Serullas. Metz. 1820. 8°	M. G. S. Serullas.
	Memorie della Reale Accademia delle Scienze di Torino. Tomo 24. Turin, 1820. 4°	Royal Academy of Sciences of Turin.
	Transactions of the Horticultural Society of London. Vol. IV. Part 4.	The Horticultural Society.
	An humble Memorial of King George III. at Longford Castle, Wilts, (an Engraving).	The Right Honourable The Earl of Radnor.
March 7.	Fœdera, Conventiones, Litteræ, et cujuscunque Acta publica inter Reges Angliæ et alios quosvis Imperatores, Reges, Pontifices, Principes vel Communitates, ab ingressu Gulielmi I^mi. in Angliam A. D. 1066 ad nostra usque tempora habita aut tractata. Vol. II. Part 2. London, 1821. F°	The Commissioners of Public Records.
14.	Esquisse du Règne Vegétale ou Tableau caracteristique des Familles des Plantes, par A. L. Marquis. Rouen, 1820. 8°	M. Marquis.
	Fragmens de Philosophie Botanique ou de la Manière la plus convenable de voir et de travailler en histoire naturelle et particulièrement en Botanique, et des Moyens de rendre cette Science plus simple et plus facile, par A. L. Marquis. 1821. 8°	———

1822.	PRESENTS.	DONORS.
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	On a new Compound Gas resulting from Animal decomposition taking place in the living Body, with some general remarks on Tympanitis. 1822. 8°	Dr. A. B. Granville.
21.	Histoire de l'Astronomie Moderne, par M. Delambre. Paris, 1821. 2 Vols. 4°	M. Delambre.
	Flora Batava. No. 60. 4°	H. M. The King of the Netherlands.
	Tracts on Vaults and Bridges. London, 1822. 8°	Mr. Samuel Ware.
	Memoirs of the Astronomical Society of London. Vol. I. London, 1822. 4°	The Astronomical Society.
	A New Method of Solving Equations with Ease and Expedition, by Theophilus Holdred. London, 1820. 4°	M. T. Holdred.
April 18.	Die Phosphorescenz der Körper oder die im Dunkeln bemerkbaren Lichtphænomene der anorganischen Natur—durch eine Rieke einiger Beobachtungen und Versuche geprüft und bestimmt von Placidus Heinrich. 4 part. Nuremberg, 1820. 4°	M. P. Heinrich.
	A Complete Course of practical Geometry, including Conic Sections, and Drawing, treated on a principle of peculiar perspicuity, by C. W. Pasley, Royal Engineers. London, 1822. 8°	Lieut. Col. C. W. Pasley.
	The Use of the Blow-pipe in Chemical Analysis, and in the Examination of Minerals, by J. J. Berzelius, translated from the French of M. Fresnel, by J. G. Children. London, 1822. 8°	J. G. Children, Esq.
	Annales des Mines. Part 4, for 1821. 8°	Le Conseil des Mines.
25.	On the Places of 145 new double Stars, by Sir W. Herschel. 4°	Sir William Herschel.
May 2.	The Hunterian Oration in Honour of Surgery, and in Memory of those Members by whose Labours it's celebrity has been advanced; instituted by the Executors of John Hunter; delivered in the Theatre of the College, February 14, 1822. London, 1822. 4°	Sir Everard Home, Bart.
9.	Beitrage zur chorographischen Kenntniss des Flussarbeits der innerste in der Furstenthumern Grubenhagen und Hildesheim, mit besonderer Rucksicht auf die Veranderungen die durch diesen Strom in der Beschaffenheit des Bodens und in der Vegetations bewerkt worden sind. Eine Erlst Anlage zur Flora des Konigreichs Hannover, von G. F. W. Meyer. Gottingen, 1822. 2 Vols. 8°	M. G. F. W. Meyer.
	Remarks on the present defective State of the Nautical Almanac, by Francis Baily. London, 1821. 8°	Francis Baily, Esq.

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May 16.	Memoirs of the Imperial Academy of St. Petersburgh. Vols. I. II. III. and IV. In the Russian Language. 3 Vols. 8°.	The Imperial Academy of St. Petersburgh.
	Practical Observations on the Nautical Almanac and Astronomical Ephemeris, by James South, Esq. F. R. S. London, 1822. 8°	James South, Esq.
23.	Statutes of the Realm. Vol. IX. 1822. F°	The Commissioners of Public Records.
	Supplement to the IVth Volume of the Transactions of the Horticultural Society. London, 1822. 4°	The Horticultural Society.
	Reasons why the Bank of England ought not to reduce the Rate of Discount to 4 per cent., by an Impartial Observer. 2nd Edition. London, 1822. 8°	The Author.
	Exposition of the Real Causes and Effective Remedies of the Agricultural Distress, by an Impartial Looker-on. London, 1822. 8°	———
	The Agricultural Case and Cash Payments briefly considered in a Letter from the Continent to a Friend in England. London, 1822. 8°	———
	A New Theory of the Tides, shewing what is the immediate Cause of the Phænomenon, and which has hitherto been overlooked by Philosophers, by Captain Forman, R. N. London, 1822. 8°	Captain Forman.
June 6.	Travels in Syria and the Holy Land, by the late John Lewis Burckhardt, published by the Association for promoting the Discovery of the Interior Parts of Africa. London, 1822. 4°	The African Association.
	Outlines of the Geology of England and Wales, with an Introductory Compendium of the general Principles of that Science, and comparative View of the Structure of foreign Countries, by the Rev. W. D. Conybeare and William Phillips. Part 1. London, 1822. 8°	The Rev. W. D. Conybeare, and William Phillips, Esq.
	Transactions of the Cambridge Philosophical Society. Vol. I. Part 2. 4°	The Cambridge Philosophical Society.
	Discours sur la Lecture fondamentale des Langues, par le Conseiller de Cour Goulianoff. Paris, 1822. 8°	M. Goulianoff.
	Astronomical Observations made at the Radcliffe Observatory, Oxford, from the 1st May, 1821, to the 1st May, 1822. (MS.)	The Trustees under the will of the late Dr. Radcliffe.
	Wellington, Poemetto del Marchese Antonio Solari Veneziano;—and Wellington proved to be the greatest Warrior of Ancient and Modern Times, by Catherine Hyde Solari. London. 8°	The Marchioness Solari.
13.	Sermons, practical and occasional, Dissertations, Translations, including new Versions of Virgil's Bucolica, and of Milton's Defensio Secunda, Seaton Poems, &c. &c. by the Rev. Francis Wrangham. London, 1816. 3 Vols. 8°	Rev. Francis Wrangham.

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June 13.	Ristretto di Fatti acaustici di Giovanni Dall' Armi letto in Accademia de' Lincei, Roma 1821. Edizioni litografica autografa. Fo. con Appendice letto già col Corredo degli Experimenti all'Accademia de' Lincei di Roma, ne' giorni 27 September e 4. Ottobre, 1821. Roma, Gennajo, 1822.	M. G. Dall' Armi.
20.	Observationes Medicæ ad Heberdeni Commentarios accommodatæ a Richardo Burney, A. B. Cambridge, 1822. 8°	Dr. Richard Burney.
	Carte de la Grèce Moderne dressée sur les Mémoires de M. Pouqueville et d'autres Voyageurs, et appuyée sur les Observations de plusieurs Navigateurs, par J. B. Barbie du Bocage, 1821. 1 sheet.	M. Barbie du Bocage.
27.	Star Tables, No. 2 for the year 1823, by Thomas Lynn, of the Honourable E. I. Company's Service, 1822. 8°	Mr. T. Lynn.
	Solar Tables for Working the Longitude by Chronometer, or for finding the Latitude by double Altitudes of the Sun or Stars. London, 1822. 8°	———
July 4.	Mémoire sur la Gelatine, par A. Michelot. Paris, 1822. 8°	M. Michelot.
	Testimonials presented to the Trustees of the British Museum in behalf of William Swainson.	William Swainson, Esq.
	The Naturalist's Guide for Collecting and Preserving subjects of Natural History and Botany, both in temperate and tropical countries, particularly Shells. London, 1822. 12°	———
	Message from the President of the United States, transmitting a Report of W. Lambert, on the Subject of the Longitude of the Capital of the United States. Washington, 1822. 8°	H. E. the American Ambassador.

Periodical Publications.

A Journal of Science, Literature, and the Arts. No. 23, to No. 26. 8°	The Managers of the Royal Institution.
Annals of Philosophy. New Series. No. 8 to 19. 8°	Richard Phillips, Esq.
The Philosophical Magazine. No. 279 to 290. 8°	Dr. Alexander Tilloch.
The Monthly Review enlarged, from July 1821, to June, 1822, with Appendices to Vols. XCV. XCVI. and XCVII.	Mr. G. E. Griffiths.
The European Magazine, from July, 1821, to June, 1822. 8°	The Proprietors.
The Monthly Censor. No. 1 for June, 1822.	Messrs. F. C. and J. Rivington.

In the List of Presents in the Philosophical Transactions for 1821, *page* 443,

For a Treatise on Heat, Flame, and Combustion, by T. H. Pasley. London, 1820. 8° Donor *Lieut. Col. Pasley*, read Donor *Lieut. T. H. Pasley.*

ERRATA.

Page 320, line 22, for *acid*, read *acids*.
—— 325, — 27, for *Pays*, read *Puy*.
—— 327, — 26, for *namely*, read *merely*.

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